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OF
PSYCHOLOGY

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THE
BRITISH JOURNAL
OF
PSYCHOLOGY

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CONTENTS OF VOL. VI.

Part 1. June, 1913.

	PAGE
The nature and development of attention. By G. DAWES HICKS .	1
The psychology of visual motion. By HENRY J. WATT . .	26
The comparative method in psychology. By CARVETH READ .	44
Some observations on local fatigue in illusions of reversible perspective. By J. C. FLÜGEL. (One Diagram)	60
Binocular and uniocular discrimination of brightness. By SHEPHERD DAWSON. (Six Figures)	78
The quantitative investigation of higher mental processes. By STANLEY WYATT. (Four Figures)	109
Publications recently received	134
Proceedings of the British Psychological Society	136

Part 2. October, 1913.

Are the intensity differences of sensation quantitative? I. By CHARLES S. MYERS	137
Are the intensity differences of sensation quantitative? II. By G. DAWES HICKS	155
Are the intensity differences of sensation quantitative? III. By HENRY J. WATT	175
Are the intensity differences of sensation quantitative? IV. By WILLIAM BROWN	184
The aesthetic appreciation of musical intervals among school children and adults. By C. W. VALENTINE	190
Note on the probable error of Urban's formula for the method of just perceptible differences. By GODFREY H. THOMSON . .	217
The effects of 'observational errors' and other factors upon correlation coefficients in psychology. By WILLIAM BROWN. (One Diagram)	223
The main principles of sensory integration. By HENRY J. WATT .	239
Publications recently received	261

Parts 3 and 4. February, 1914.

	PAGE
Freud's theory of the unconscious. By WILLIAM BROWN. (One Diagram)	265
The analysis of some personal dreams, with reference to Freud's theory of dream interpretation. By T. H. PEAR. (Seven Figures) .	281
The conditions of belief in immature minds. By CARVETH READ .	304
An experimental investigation of perception. By FRANK SMITH .	321
The colour perception and colour preferences of an infant during its fourth and eighth months. By C. W. VALENTINE. (One Diagram)	363
The testimony of normal and mentally defective children. By T. H. PEAR and STANLEY WYATT	387
The conditions which arouse mental images in thought. By CHARLES FOX. (One Diagram).	420
On changes in the spatial threshold during* a sitting. By GODFREY H. THOMSON. (One Diagram)	432
Review	449
Publications recently received	453
Proceedings of the British Psychological Society	455



LIST OF AUTHORS

	PAGE
BROWN, WILLIAM. Are the intensity differences of sensation quantitative? IV.	184
BROWN, WILLIAM. The effects of 'observational errors' and other factors upon correlation coefficients in psychology	223
BROWN, WILLIAM. Freud's theory of the unconscious	265
DAWSON, SHEPHERD. Binocular and uniocular discrimination of brightness	78
FLÜGEL, J. C. Some observations on local fatigue in illusions of reversible perspective	60
FOX, CHARLES. The conditions which arouse mental images in thought	420
HICKS, G. DAWES. The nature and development of attention. . .	1
HICKS, G. DAWES. Are the intensity differences of sensation quantitative? II.	155
MYERS, CHARLES S. Are the intensity differences of sensation quantitative? I.	137
PEAR, T. H. The analysis of some personal dreams, with reference to Freud's theory of dream interpretation	281
PEAR, T. H., and WYATT, STANLEY. The testimony of normal and mentally defective children	387
READ, CARVETH. The comparative method in psychology	44
READ, CARVETH. The conditions of belief in immature minds . . .	304
SMITH, FRANK. An experimental investigation of perception . . .	321
THOMSON, GODFREY H. Note on the probable error of Urban's formula for the method of just perceptible differences	217
THOMSON, GODFREY H. On changes in the spatial threshold during a sitting	432
VALENTINE, C. W. The aesthetic appreciation of musical intervals among school children and adults	190
VALENTINE, C. W. The colour perception and colour preferences of an infant during its fourth and eighth months	363
WATT, HENRY J. The psychology of visual motion.	26
WATT, HENRY J. Are the intensity differences of sensation quantitative? III.	175
WATT, HENRY J. The main principles of sensory integration. . .	239
WYATT, STANLEY. The quantitative investigation of higher mental processes	109
WYATT, STANLEY, and PEAR, T. H. The testimony of normal and mentally defective children	387

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL
SOCIETY

	PAGE
Meetings on March 8, May 3, 1913	136
Meetings on June 7, June 8, November 8, 1913, January 24, 1914 .	455

THE BRITISH JOURNAL OF PSYCHOLOGY

THE NATURE AND DEVELOPMENT OF ATTENTION.

By G. DAWES HICKS.

- I.
 1. *The two opposed classes of theories of attention.*
 2. *The error underlying both of them.*
 3. *In both there is wanting sufficient recognition of the early stages of mental development.*
- II.
 1. *Attention and apprehension.*
 2. *Distinction of the act or process of apprehending and the content apprehended.*
 3. *The rudimentary or primitive act of apprehension.*
 4. *Apprehension is always a process of discriminating and comparing.*
- III.
 1. *The common or essential characteristics of attention.*
 2. *The rudimentary or primitive act of attention.*
 3. *The nature of clearness and distinctness.*
 4. *The importance of movement-factors in primitive attention.*
 5. *Secondary attention. The nature of 'interest.'*
 6. *Voluntary or deliberate attention. The 'sense of effort.'*
 7. *Conclusion.*

"ATTENTION," said Ebbinghaus, "is a veritable embarrassment of psychology. In some even comprehensive treatments of the mental life it has still up to the most recent time been as completely neglected, as for the most part it had been neglected in the English association-psychology. In others it appears to be inserted in the whole system in the strangest, now and again it may be said in the most helpless, fashion. That under these circumstances great divergence prevails both in the presentation of its features and in the more detailed

2 *The Nature and Development of Attention*

accounts of its nature is intelligible. As compared either with the ordinary use of the term or with the inner relatedness of the facts appropriately grouped together as facts of attention, the majority of these accounts are too narrow; they are too inclined to take into consideration but one side of the matter or something which is only to be met with under special conditions¹. These remarks may fittingly serve as a preface to the following attempt to traverse once more a field that may seem to have been already sufficiently explored, and from which attempt, it may be thought, little or nothing of fresh importance is likely to accrue. In spite of much to the contrary that has been written of late, I venture, however, to plead that the subject calls for the mode of treatment I have here in view. An account, such as that presented by Titchener², of the experimental work that has been done on attention exhibits only too plainly the barrenness in psychological significance of what has thereby so far been achieved, and illustrates also how intimately the value of what is yielded by experiment depends upon the theoretical standpoint from which specific problems are framed and their solution sought. There is no possibility of entering upon an investigation of the conditions and laws of attention without some conception of the nature of the process itself, and if that conception be not reached through a careful analysis of the facts of the mental life, the alternative will be the acceptance of the crude uncritical generalisations of popular reflexion with which, in that case, every interpretation of the experimental results will be saturated.

I.

§ 1. A variety of ways of handling the facts of attention has become familiar in the history of psychology. In the long run, however, they will all be found to fall under one or the other of two broadly contrasted modes of explanation which Lotze characterised in a well-known passage³. On the one hand, attention has been depicted after the fashion of a varying power of illumination, a sort of waxing and waning light, which may be directed by the mind upon the several presentations it receives, and in accordance with the variations of which, other things remaining the same, will be the clearness, the precision, the completeness in apprehension, of what is attended to. Lotze himself remarks upon the little help there is to be got from a view of this kind. By

¹ *Grundzüge der Psychologie*, 1. 2^{te} Aufl. 1905, 611.

² *The Psychology of Feeling and Attention*, 171 sqq.

³ *Metaphysik*; Buch iii., *Psychologie*, § 273.

concentrating attention upon presentations we are said to increase their intensity, their clearness, their distinctness. But a mere gazing at anything, even though it be heightened to infinity, would in itself be powerless to bring about this result. A mere putting forth of energy, supposing an intelligible meaning could be found for such an expression, would be utterly fruitless in accounting for the effects produced, unless it were shown to be a means of bringing the content in question into comparison with and relation to the elements surrounding it. On the other hand, attention has been held to be no more than a name for describing the varying degrees of intensity which the several presentations and re-presentations entering into the mental life themselves possess,—a name, that is to say, for indicating a property or attribute of the presentations or re-presentations themselves, in virtue of which they secure special notice on the part of the apprehending subject, not a property accruing to them in and through the act of attending. The presentations are regarded as rising, so to speak, into consciousness, through their own strength; they are supposed to act and react upon one another, to blend or fuse with one another, to inhibit one another, to revive one another, and so forth, and the growth in clearness, distinctness, liveliness, of the predominant presentation is accordingly traced back to mutual influences of the kind mentioned. In short, the energy or activity which on the former view was ascribed to the process of attending, is transferred on this view to the contents attended to, and, in accordance with the dictum that “psychology is concerned with nothing beyond presentation and its laws,” the only psychological explanation that can be offered of attention will be in terms of the movement, the reciprocal relations, of presentations or ideas.

§ 2. These two modes of contemplating the phenomena in question are usually considered to be essentially antithetical, and so, no doubt, in many respects they are. But antithetical positions frequently participate in a common error, and I think we have now before us an instance in point. The error which, as it seems to me, lies at the root of both methods of procedure, and which is responsible for most of the perplexities to which they give rise, consists in treating presentations, contents of apprehension, as objects. I can prepare the way for what is to follow by dwelling for a moment upon this initial matter. One of the disadvantages appertaining to the use of the term ‘presentation’ is the difficulty, well-nigh the impossibility, of freeing that term from the incidental significance which has come to attach to it on the strength of our relatively mature experience. In mature experience we have certainly

come to look ordinarily upon the act of apprehending as distinct and separate on the one hand from the real thing and on the other hand from what we call the presentation of the real thing. We habitually distinguish, for example, the act, or process, of hearing both from (say) the vibrating string of a violin and from the sound heard. I have no quarrel with this distinction which, so far as it goes, is legitimate enough. But the distinction may have, and I should say obviously has, a very different significance in the two cases just indicated. From a psychological point of view, at any rate, we are entitled to say that the act of apprehending is one existing fact, and that the vibrating violin string is another and a separately existing fact. But are we likewise entitled to say that the act of apprehending is one existing fact and that the presentation, the content of the act of apprehending, is another and a separately existing fact? For reasons that will presently appear, I reply to that question without hesitation in the negative. Meanwhile, however, it will be well to draw out certain implications of such negative answer. According to a sufficiently prevalent mode of looking at the matter, what takes place when a sound is perceived would be described somewhat as follows. The vibrating body, by transmitting its movements to the air, affects the auditory end organs, a certain change in the condition of the auditory nerves and the cerebral centres with which they are connected ensues, and in consequence there arises in the mind an auditory presentation, which by a mental act directed upon it is apprehended or attended to. The auditory presentation, that is to say, will exist as a reaction or modification of the mind, whether it be apprehended, whether it be attended to, or not. The presentation will be one mental fact, the act of apprehending or of attending will be another mental fact. Now, the negative answer just given to the question proposed involves the rejection of this interpretation. I leave out of account here, as irrelevant to the present issue, the problem around which much discussion has recently centred, whether namely a secondary quality, such as a sound, is rightly described as mental, or as physical, or as neither mental nor physical. I confine myself to the specific set of occurrences involved in the perception of a sound, and roughly the analysis I propose to substitute for the foregoing is this. On the occasion of the vibrations of air, which emanate from the vibrating violin string, affecting the auditory nerves and thereby the appropriate cerebral centres, there arises, whether as effect or correlative or in virtue of whatsoever relation between body and mind be postulated, not forthwith a presentation, not forthwith a sound, but a state or process

of consciousness, an act of apprehension, which, in this case, is the act of distinguishing or recognising a sound. The auditory presentation, that is to say, is the content of the act of apprehension and has no separate existence, as an independent reaction of the mind, prior to the act of apprehension, and upon which the act of apprehending is, so to speak, subsequently directed. There is no having a presentation apart from the process of apprehending itself. The distinction, important though it be, between the act of apprehending and the content apprehended, does not, therefore, warrant the assumption of two separate mental facts or occurrences. Such, however, is precisely the assumption which, either tacitly or avowedly, is made when presentations are treated as objects. Presentations, so conceived, have unavoidably conferred upon them a quasi-substantive mode of existence. They come to be regarded as independently existing entities which form themselves into series, group themselves, operate upon one another, and which in fact discharge the specific functions we are in the habit of ascribing to what we call things. No psychologist would, I take it, care to speak of presentations as literally 'things,' and yet surely when presentations are dealt with by aid of the notions just referred to, it would be extremely difficult to say in what respect they are supposed to differ from 'things.'

§ 3. The objection I am pressing seems, then, to me to be fatal to both the classes of theories signalised by Lotze. I urge further that each of these modes of treatment discloses, on nearer scrutiny, the bias that inevitably results from a too exclusive limitation of the problem to conscious experience of a highly developed type.

The notion of attention as a focussing of psychical energy upon the different aspects, elements and objects in the one field of consciousness is, I suppose, a notion which is naturally suggested by ordinary reflexion upon what appears to be obviously a feature of the mature mental life, —the facility, namely, which we possess of attending in various degrees to the same fact, or to what we call the same fact, and of giving a similar degree of attention to facts extremely varied. From this merely popular and superficial introspection of the higher forms of attention to the assumption of a unique and specific activity is, however, a far cry. If no better grounds can be furnished than those derived from a merely popular way of describing experiences which in themselves no one doubts, the assumption of a unique and specific activity would amount to no more than a resuscitation of the antiquated doctrine of faculties. A similar line of consideration should constrain us to the admission of a specific activity of perceiving, of imagining, of remembering, of thinking,

and so forth. And notwithstanding the disposition, on the part of some recent writers, to reinstate the old faculty psychology, no one has attempted to set aside the reasons that led Herbart, for example, to call for its unreserved rejection. Briefly those reasons were these. The doctrine has no explanatory value. It is a resort to the idea of an occult power from whose assumed mode of operation the observed changes that are accounted its effects in the contents apprehended are deduced. A simplicity, a singleness, is implied in the cause, the specific mode of psychical energy, which is altogether out of keeping with the extreme complexity of the results it is said to produce. A single agency of the kind referred to is wholly irreconcilable with the intricate manner in which mental processes are connected with one another, and is in violent contradiction with the ascertained facts of mental growth and development. Let it suffice to dwell here on the last point. Even the most cursory inspection must convince us that the states of mind called states of attending, although evincing certain common fundamental features, exhibit differences in character clearly dependent on the different stages of mental evolution at which they have made their appearance. When, for example, in a well-known passage, Hamilton distinguishes three grades or kinds of attention, (*a*) a mere vital and irresistible act, (*b*) an act determined by desire, which, though involuntary, may be resisted by our will, and (*c*) an act determined by a deliberate volition, he may be said to be mapping out three prominent phases in the historical development of mind. The conditions which determine attention in these three forms will turn out to be to a large extent of dissimilar origin. In the case of the first, they will be mainly of the mechanical order, psychological analysis will be confronted with its limit, and the explanation, hypothetical it may be, will tend to pass into the region of physiology; in the second, they will be prevailingly of the character that it is customary to include under the comprehensive term 'interest'; and finally, in the third, they will be those involved in the reflective pursuit of a deliberately conceived end or purpose. Even, then, so much recognition of difference in the way in which attention is manifested is really inconsistent with the doctrine of a specific faculty. Such other grounds as are advanced for the view are generally drawn from those unquestionable experiences of the mature consciousness in which what is called the 'effort to attend' is involved. But, in the first place, the 'effort to attend,' in the form in which we are familiar with it, is evidently an experience possible only for a mind of a highly developed type. It involves clearly anticipatory representations,

accompanying desires, the consequent repression of ideas which otherwise might form part of the sphere of apprehension at the moment, and generally certain of those motor experiences which make their appearance in any exercise of effort on our part, and which themselves can never find expression in any single species of sense-presentation. There is usually involved, further, the admittedly complex factor, consciousness of self, and it can scarcely be questioned that experiences in which the representation of self is an ingredient are of far too composite a nature to entitle us to draw any conclusion from them to the primitive stages of the activity of attending. In the second place, it has to be borne in mind that what may appear to us introspectively, in our mature experience, to be a simple and unanalysable process need not by any means necessarily be a simple and unanalysable process as a psychological fact. Mental processes of the most intricate kind repeatedly come to appear simple the more readily and habitually they are performed. If, then, attention is to be regarded as a special activity, one certainly requires better evidence for it than such as is furnished by the antithesis between what is roughly described (by Wundt, for example) as the attitude of 'passive receptivity' and the attitude of active exertion or effort. Important though this difference may be, and no one questions its fundamental importance as gradually making its appearance in the development of mind, yet, unless it can be shewn to be ultimate, it affords no presumption whatever in favour of the hypothesis we are considering.

On the other hand, the notion of attention as capable of explanation in terms of the movement, the reciprocal influence upon one another, of presentations, may be thought to arise naturally from ordinary reflexion upon the more rudimentary or non-voluntary stages of the process. And this, no doubt, to a large extent is true. As contrasted with others, certain presentations seem to have the power of thrusting themselves into prominence, of becoming 'dominant,' to use Mr Bradley's phrase, in consciousness. "A flash of lightning by night, the report of a firearm, the sudden prick of a knife, or a violent internal pain, all these for the moment so occupy our notice that everything else becomes feeble or is banished." Here, again, however, I believe we may be easily misled by trusting too implicitly what inspection of our mature experience seems obviously to yield. The facility which we possess of sharply distinguishing presentations from one another, of holding them definitely apart, of regarding them as though they were so many separate, detached, individual objects, may be an acquired facility, a facility attained by means

of the process of attention itself. If isolation, independence, definiteness of outline, be not originally given, if, as features in what is apprehended, they come about gradually in the history of the mental life, and largely through the very process we are seeking to account for,—and this, surely, is the position to which psychological analysis unequivocally leads,—then clearly these must not be presupposed as data on the basis of which a theory of attention can be framed. It must not be assumed, that is to say, either that presentations are in truth so many discrete and independent entities, or that in their original character they are objects of the mind's contemplation. For, according to the general principle just indicated, presentations will only appear to be the one or the other in consequence of there having been brought to bear upon them reflective considerations, the legitimacy or illegitimacy of which as psychological categories it will be needful to determine.

II.

§ 1. If, then, we reject on the one hand the view according to which attention is a special activity, and on the other hand the explanation of its characteristics by reference to the interaction of presentations, it remains to make the attempt to shew in what way the undoubted peculiarities of the facts of attention can be accounted for by tracing the way in which the process of attending is connected with the simpler and more primordial processes of mind. Professor Ward's extended use of the term attention "so as to include what we ordinarily call inattention" has, at least, the advantage that the essential continuity of attentive consciousness with inattentive consciousness or consciousness simply is thereby emphasized. The difference between these two forms of consciousness is, it is maintained, a difference not of kind but of degree. But true and significant though I conceive this contention to be, it would, I think, be a mistake to deprive psychological terminology of the only single expression it has for denoting a body of facts which do exhibit, under a variety of differences, certain common characteristics as contrasted with the states of consciousness which we ordinarily call those of inattention. That underlying this specific distinction there is a more fundamental and generic identity is indeed precisely the thesis I am concerned to defend, but I feel the need of a term by which compendiously to describe the set of specific features which in that case call

for explanation¹. For reasons that will immediately appear, the term 'apprehension' seems to me a suitable designation for the generically identical process which, following Professor Ward, I believe to be at work in both the contrasted phases of conscious experience, and the problem I have now to face is that of trying to shew how an intelligible account can be given of attention as a differentiated mode of this one fundamental process.

§ 2. So long as analysis is confined to what is met with at comparatively advanced stages of mental development the problem thus formulated hardly admits of successful solution. In dealing with the mature conscious life, there is always the temptation to accept as simple and ultimate what in truth may be an exceedingly complicated product, and it is just in this particular that introspective observation is almost certain to lead us astray. It is well-nigh impossible to disentangle the intricate phenomena that come before us in the higher forms of attention, unless we endeavour to work our way back from these to the earlier and less evolved phases of experience, out of which they may be conceived to have originally emerged. Pursuing that course, many considerations seem unmistakably to indicate that what is vaguely called the power of concentrating attention is a derivative function, that its ingredients are by no means invariably the same, that it is largely of the nature of a habit, and that there is no more of ultimate activity or faculty involved in it than such as belongs to the fundamental process of apprehending, of which it is a complicated and highly developed modification.

One of the considerations referred to is that to which I have already alluded. In mature experience no distinction we can draw seems more obvious and self-evident than that between the act of apprehending and the content apprehended. We unhesitatingly distinguish hearing from the sound heard, seeing from the colour seen, and so forth. And it is undoubtedly on this distinction that the view of attention as a specific unique activity is in the long run based. For here likewise nothing seems more manifestly in accordance with fact than that the act of attending and the content which results therefrom are two distinct and separate entities. Yet the slightest reflexion, as Hume would say, is sufficient to dispel the idea that this distinction, for the mature

¹ I doubt the advisability of employing such a mode of expression as 'degrees of distribution of attention' to indicate the specific features in question (cf. Ward in *Mind*, 1887, xii. 58), because it almost inevitably suggests the thought of attention as being comparable to a 'moveable light.'

consciousness seemingly so indubitable, could conceivably have entered, even in the crudest form, into the experience of the primitive mind. It is a distinction the recognition of which presupposes a superstructure of reflexion such as is utterly beyond the scope of the early mental life, and can only be possible for a mind that has attained, in some measure, at least, to the stage of self-consciousness. We distinguish the content from the act of apprehending, but what in our mature experience gives stability and definiteness to the content as thus distinguished is the presence of a number of thoughts or concepts which connect the content in question with the objective order of real fact. So too we distinguish the act of apprehending from the content apprehended, and what gives stability and definiteness to the distinction is a number of thoughts or concepts which connect the act of apprehending with the train of experiences constituting what we call the self. It is quite true that even a mind of the simplest type must contain within itself the indispensable condition of the distinction which subsequently makes its appearance in the course of the successive stages of mental evolution. It is quite true that psychical existence has precisely the peculiarity of exhibiting in all its states a unique double-sidedness of aspect; its states are, that is to say, from the very first, states *in* and *through* which the subject is aware, and aware *of* something. And I anticipate the objection that the content may as a matter of fact be separate from and independent of the act of apprehending even though it be not recognised as thus separate and independent by the experiencing subject. But the point is that the start is bound to be made from an act of apprehending which is such only as the awareness of a content. To maintain that the two are separate even though they are not recognised as separate does not help in the least to account for the gradual emergence of the distinction as a recognised distinction. It is still from the concrete fact, an awareness of a feature or features which as defining the act of awareness we call the content of it, that any such account must proceed, and even though there were an actual severance between act and content, such actual severance would avail us not at all in tracing the way in which the distinction in question comes to be drawn by the conscious subject. Just because, therefore, the distinction cannot be present as a recognised distinction in primitive experience, the fact of its recognition in mature experience does not warrant the conclusion we are too hastily inclined to rest upon it, that the act of apprehension is one existing entity, and the content of that act, or the presentation, another. In other words, the assumption of a primitive

severance in fact between act and content seems to me to be "an illegitimate transference," as Adamson puts it, "to the supposed original condition of the inner life of a distinction which has definite meaning only in its later form¹," that is, in the form in which it points to the contrast between the act of apprehending and a real thing apprehended².

§ 3. The rudimentary act of apprehension, from which conscious life may be supposed to start, would not, so far as I can see, be fitly described as a state either of cognition or of feeling or of conation. After all, these terms are generalities, and serve mainly to indicate certain broad differences which are recognised in mature experience and which have become fixed in our ordinary nomenclature. They name with sufficient accuracy three lines along which the mental life develops, each of which reaches, by degrees, a certain measure of independence of the others. But the meaning assigned to these terms is determined wholly by our knowledge of the processes in the developed stage, and it is *prima facie* extremely improbable that an equal degree of independence could be claimed for the three attitudes of mind as evinced in less developed stages. Cognition, in the current acceptation of the term, certainly carries with it, as part of its significance, recognition of the antithesis between subject and object, an antithesis which, I have tried to shew grounds for thinking, cannot be included as from the outset among the facts of which there is consciousness. So, too, feeling, as originally experienced could not involve the highly characteristic feature which it afterwards acquires,—definite reference, namely, of any particular state of feeling to the experiencing subject, a feature which colours the connotation of the term as it is ordinarily employed. Once more, the characteristics usually ascribed to conation, as essentially involving the representation of an 'is to be' in contrast with what actually is, are obviously, I should say, not characteristics that belong to the early experience of impulse and bodily movement. So far, then, as the rudimentary states of mind are concerned, all we are entitled, I

¹ Lectures on the Principles of Psychology in Vol. II of the *Development of Modern Philosophy*, 182.

² The assumption really means, of course, that the relation between the act of apprehension and the external thing is simply being reduplicated in the relation between the act of apprehension and its content. I agree with Professor Alexander (this *Journal*, IV. 244) that, as thus conceived, what is called content blurs the relation of act and that which is apprehended. The content becomes, then, that which is apprehended, and, in order to render *such* apprehension intelligible, we should still have to regard it as also possessing the aspect which I am denoting by 'content.'

think, to say is that they would contain within themselves the roots from which these three diverging stems take their origin. One may venture to lay down the general proposition that the nearer we approach the beginning of consciousness, the less apparent will be the distinction of cognition from feeling and striving. Increasing indistinctness of presentation need not merge presentation into feeling, but in the early life of mind any prominent difference between a sense-feeling and a sense-presentation would seem to be precluded. The elementary components of mind may, then, not inappropriately be described as in essence acts of apprehension, the contents of which may be of varied kinds, either those which come to be distinguished as sense-qualities, or those which may come to be experienced as sense-feelings, or those which become impulses or strivings and find a terminus in bodily movement. Without, therefore, assigning to cognition, in the ordinary significance of the term, a supreme position, it may, I think, be contended that the functions which find their most complete realisation in cognition are the common conditions of all phases of inner experience. Feeling and striving may have equally with cognition their direct antecedents in the primitive processes of mind, but it would be an error to ignore their character as dependent upon acts of apprehension, and to credit them with a substantive existence such as in no wise, at any stage of development, really belongs to them. It is only as based upon acts of apprehension that feeling and striving can be said to be mental facts at all.

§ 4. Can we characterise further the acts of apprehension which are thus conceived to constitute the very essence of mind? I think we can. With the fine discernment that often gives to his psychological work a far-reaching suggestiveness, Aristotle insists, in more than one section of the *De Anima*, upon regarding αἴσθησις not as mere passive receptivity, but as a δύναμις κριτική and each specific mode of αἴσθησις as a μέσον κριτικόν. In other words, Aristotle refuses to allow that sense-presentations and their images can be rendered explicable without some element of that discriminative activity which is usually supposed to be the distinctive feature of the higher mental processes alone. Sheltering myself here under an authority which no modern psychologist need be ashamed to own, I venture to urge that discrimination or comparison is involved in having even the crudest, even the most indefinite, sense-content to which the name presentation can be assigned. A very obvious objection to which a contention of this sort is exposed may be at once forestalled. It is quite true that

the terms 'discrimination' and 'comparison,' as ordinarily employed, indicate a reflective operation of an intricate kind with the capacity for which it would be ridiculous to endow a primitive mind. After discarding, on account of their implications, various conceptions by the aid of which the first beginnings of consciousness have been represented, it would certainly be a gross inconsequence, on my part, to picture those first beginnings as in reality processes of thinking. No such absurdity is intended. It is not meant that in order to be aware of a sense-quality, *A*, it is necessary that a conscious subject should perform the mental operations expressible in the propositions "*A* is different from *B*," or "*A* resembles *C*." Far from it. That would involve the use of the abstract ideas of likeness and difference,—ideas which manifestly are only possible for a self-conscious mind. But, in our own experience, differences and resemblances are recognised long before we have any conception of difference or resemblance. Or to put it otherwise, *A* may be distinguished from *B*, long before the precise features in which they differ are appreciated. This more rudimentary process of differentiating and distinguishing may itself be looked upon as exhibiting any number of varying grades or stages, and I see no difficulty whatever in supposing that in its crudest form discrimination lies at the root of the earliest manifestations of consciousness. I, at any rate, can find no means of realising what a state of mind could be which does not involve at least some faint, confused, indefinite recognition of a content possessing a distinguishable character. If, then, every act of apprehension be described as an act of discriminating, assimilating and comparing, no violence is really done to the consideration of the relatively extreme simplicity and narrowness of range that admittedly must be held to be characteristic of the mental lives that are lowest in the scale of evolution. Whilst, on this view, it is true that the *general character* of apprehension, whether it be the first dim obscure strivings of sense or the highly elaborate exercise of abstract reasoning, remains the same, yet the *specific modes* of its exemplification are infinitely various, each stage of its advance being dependent in the last resort upon increase of facility in distinguishing and relating the elements presented in the world of concrete fact in the midst of which a conscious life is lived. Apprehension, so conceived, may certainly be said to be an 'activity'; every phase of conscious life, as thus interpreted, is a state or condition in which the subject is 'active.' But 'activity' does not here signify a mysterious unlocking of force or putting forth of energy; it points rather to a certain aspect of what has been called discriminating and

comparing,—to its aspect, namely, as a process, or occurrence, in time. I imagine, however, that, as applied to mind, the general term ‘activity’ is chiefly of service in negatively excluding misconceptions, such as that of passive receptivity, than in positively throwing light upon the nature of consciousness.

III.

§ 1. What explanation, on the basis of the foregoing account of the structure of the primitive mental life¹, can now be offered of the process of attention in the sense in which we have agreed to understand that term? The view which has been taken is clearly incompatible with the notion of an abrupt introduction of any unique power or faculty into the constitution of mind. It will not be as the effect of the exercise of any special activity, but as a consequence of the conditions under which the processes of the mental life develop that we shall seek to render explicable the results which by general agreement are called results of attending.

The common or essential characteristics, evinced in all processes of attention, call first for consideration. They may be said to be these: (a) a certain selection or limitation within the field of what is apprehended of some feature or features and the relative neglect or disregard of the rest; (b) the increased clearness and distinctness of the content attended to, its greater vivacity and prominence, its more precise and adequate appreciation; and (c) the experiences in the form of feeling-tone that sometimes precede and sometimes accompany the attitude of attending.

§ 2. I start by dwelling on the last of these characteristics. It can hardly be doubted—few psychologists, I think, ever have doubted—that the earliest changes in the contents of apprehension which entitle such contents to be described as facts of attention must be initiated by differences in the intensity of feeling-tone accompanying some particular content apprehended. A content possessing a higher degree than others of painful or pleasurable feeling-tone would become naturally differentiated from the rest; there would be a natural tendency for other contents of a less pleasurable or less painful character to be excluded from the sphere of consciousness. It has, indeed, been argued

¹ I have worked out in detail what I have only cursorily outlined above in various contributions to the Aristotelian Society; see *Proceedings*, N.S. vi. 271; viii. 160; and x. 232.

that to lay stress on this consideration is to overlook what is alleged to be the fact that in our own experience pleasure or pain never comes until after the object has been attended to, and that therefore such pleasure or pain cannot give rise to the act of attending. We can, it is contended, be neither pleased nor displeased by what is not yet in consciousness¹. But the argument misses the whole point of the consideration, which, of course, is that a content may be 'in consciousness' before it is attended to, and unless that be admitted, it is difficult to see how any psychological explanation of the circumstances we are now concerned with is possible at all. I take it, then, that experience in the form of feeling-tone can and often does precede the act of attending in the strict sense of the term. At first, the variation in the intensity of feeling-tone could produce no more than the purely mechanical effect of detaining for a longer time than would otherwise have been the case the content in question before the mind. By the mere fact of its being thus singled out, so to speak, from among the other transient contents of apprehension, its presence in consciousness would be prolonged; using, if one may, a spatial figure, it would tend to spread itself over the field of consciousness at the expense of the other contents, and within certain limits increase of duration would contribute to producing the results for which we are trying to account. For retention for a longer time in consciousness would in its turn give rise to a two-fold effect. It would afford, on the one hand, a greater opportunity for the establishment of associations between the content in question and other contents apprehended by the conscious subject. It would afford, on the other hand, a greater opportunity for easy and rapid reproductions, in the form of re-presentations, or images, of the said content.

As regards the former effect, one appeals to the familiar fact that a certain duration of the sensory stimulus is necessary in order that an act of apprehension should take place at all. I do not suppose that the relation between duration of stimulus and completeness of apprehension is by any means a simple one. In our own experience, for example, duration of even a comparatively intense stimulus beyond a certain period is unfavourable to apprehension. But there is no reason to doubt that within normal limits, a longer duration of stimulus does afford opportunity for a more complete comparison of the content with other contents and of its more accurate discrimination from them. In like manner, an increase in the intensity of the stimulus will have as

¹ W. B. Pillsbury, *Attention*, 56 and 287.

its concomitant, besides an increase in the intensity of the content apprehended to which I shall refer later on, a larger measure of feeling-tone, and this, in accordance with what has already been said, would also make for the results just indicated.

As regards the latter effect, every reproduction of the apprehended content must obviously afford additional opportunity for increased precision of discrimination. Each specific appearance of a revived content would enable it to be marked off with greater definiteness of outline from what was simultaneously present at the moment, and every increase of definiteness of outline would render possible an increased precision when a renewed content of like kind was apprehended. For one thing, the general setting of the latter would be no longer identical with what it was on the occasion of the previous appearances. And this new setting would serve as a means for instituting fresh comparisons, so that repetition would necessarily bring about, although by no means in uniform fashion, increased clearness and distinctness of what was revived or reproduced.

§ 3. There is, so far as I can see, no way of accounting for the clearness and distinctness that constitute the principal result of attending except by thus connecting the whole process of attending with the elementary function of apprehension—with the act, namely, of discriminating, of gradually recognising the marks whereby one content is contrasted with or assimilated to another. As I have urged above, clearness and distinctness would not be produced either by a mere increase of mental energy directed upon a limited portion of the field of consciousness, or by a mutual interplay of presentations, conflicting with and reinforcing one another. The difficulty that confronts the latter hypothesis is well illustrated in Titchener's treatment of the matter. Despairing, apparently, of finding any means of solving the problem in terms of the reciprocal action and reaction of presentations, Titchener boldly cuts the knot by declaring that clearness is an independent attribute of sensation, which, within certain limits, may vary independently of the other concurrent attributes, such as quality, intensity and duration¹. It is one thing, however, to cut a knot and another to untie it. On the one hand, Titchener is unable to doubt that "some sensible qualities are, intrinsically, clearer than others," he inclines to the view that "differences of clearness are, like intensive differences, ultimate and distinctive," and he considers clearness to be "an attribute of sensation, conditioned upon nervous predisposition,

¹ *Op. cit.* 171 sqq.

just exactly as quality is an attribute of sensation, conditioned upon nervous differentiation." On the other hand, he admits that one and the same sense-content may vary in clearness, according to the degree of 'concentration' of attention, he places novelty, rarity, unaccustomedness among the conditions of clearness, and he recognises the great importance of the associative relationship "between the sensation and the whole circle of ideas dominant at the moment" as determining the clearness of the former. I know not how these two portions of the theory are to be wrought together into a coherent view. If clearness be an ultimate and intrinsic property of sense-contents it will hardly do to speak of clearness as purely relative in character, dependent on a number of conditions extraneous to the content of which it is a property. I confess I am unable to attach any intelligible meaning to the former of these contentions. It is evidently based upon the assumption that isolated sense-contents are already given, prior to apprehension, endowed with all the characteristics which are described as attributes, clearness among the rest. The rejection of that assumption leads, it seems to me, directly to the view of clearness upon which I have been proceeding. If conscious experience does not start with a multiplicity of sense-presentations, each definitely marked off and separated from the rest, but with a vague, ill-differentiated whole, out of which by successive acts of discriminating there gradually emerge definite presentations, then it must follow that the clearness and distinctness of such presentations will depend upon the number of marks or features which the conscious subject is able to discern in each, and it will only be in and through the process of discriminating that the presentation will have clearness, or indeed any individuality of character at all. A sense-content, in short, increases in clearness just in proportion to the number and kind of distinctions that contrast with or resemblance to other presentations has enabled the discriminating subject to constitute.

§ 4. Another factor calls for notice, a factor, moreover, that will be seen likewise to make for the result we have had in view. I refer to the intimate connexion of elementary sense-experiences with bodily movements. Every stimulation of the sensory mechanism has mechanically connected with it a certain stimulation of the motor mechanism; the junction between the two is so close and uniform that hardly any change in the one fails to find a response in the other. It is with the elementary experiences, hard to discover psychologically, which precede and accompany the execution of movement that we have here specially to do. The physiological conditions of sense-apprehension themselves

evoke to a greater or less extent certain efferent discharges causing muscular contractions as a natural fact. With recurring sense-contents more or less resembling one another, there will come, therefore, to be associated, though in various degrees, the experiences that have accompanied the movements thus initiated. And in accordance with the character in part of the sense-contents and in part of the movements associated with them will be the prominence of the former in consciousness. The sense-contents accompanied with a large quantity of the experience of movement will naturally retain a more prominent place, and if they incite at the same time feelings of pleasure or more specially of pain, and if they give rise to motor experiences that are related to the continuance of pleasure or the removal of pain, they will secure relatively the largest place in consciousness. Thus I conceive that in a general way we can understand how it is that with the characteristics of attention, the retention for a longer period in consciousness of an apprehended content and the increase of clearness and distinctness of that content which ensue through its being compared with and related to other contents, there should naturally come to be associated those experiences of strain or tension which manifest themselves so pronouncedly when will, in the strict sense of the word, has been developed, and the conscious subject can control series of movements or even trains of thought.

§ 5. So far I have been dealing with what can appropriately enough be called primary attention. I pass now to secondary attention, if one may adopt that term for the process as it is exhibited at the stage when a definite consciousness of a world of external objects has been attained, but which is still more or less now voluntary in character. I think it will be found that the principle upon which I have been proceeding is adequate here also to render a satisfactory explanation of the facts. A reference to some familiar experiences will aid us in carrying on our inquiry.

In the first place, I point to the well-established fact that certain limits are imposed by the range of experience already possessed by a conscious subject upon the kind of objects which can be for him possible objects of attention. In other words, the same fact presented to differently equipped minds will not be attended to in the same way. The objects of sense-perception, to which for the present we can confine our inquiry, exhibit a great diversity of qualities, and some of these qualities seem inevitably to force themselves into notice, to attract or engross, as it is said, attention. Often this feature of attractiveness or

fascination on the part of an object has been regarded as the distinctive feature of so-called non-voluntary or automatic attention in contradistinction to voluntary or deliberate attention. But when once allowance is made for the circumstance just alluded to, the characteristic ceases to be one that can be fixed upon as differentiating, in any decided manner, the two modes of attention in question. The features or aspects which 'attract' or 'engross' the attention of one conscious mind will not, and we may safely say cannot, 'attract' or 'engross' the attention of another mind which we will describe, in popular language, as "differently constituted." Whilst halting on a certain occasion in front of one of the most beautiful views obtainable of Grasmere and its hills, a lover of the Lake-country was accosted by a Manchester tripper, and had to find an answer to the query, "Is there *anything* to see in this place?" The proverb, the eye only sees what it brings with it the power of seeing, needs no better illustration. The limitation to which the proverb refers depends psychologically upon the possibility of recognising in the new material presented features capable of being assimilated with the stock of ideas, the particular trains of experience, already possessed, so to speak, by the individual subject. For the most part we attend, to use the orthodox mode of expression, only to that which 'interests' us. Interest, however, is not, as one might be tempted to imagine from much that has been written about it, an unique and unanalysable relation between consciousness and its object. The term is but a convenient symbol for indicating the complex set of conditions to which I have been referring. "It is quite certain," says Mr Bradley, that interest "consists to a large extent in pleasure and pain¹," and I have no desire to call in question that assertion. But I think it chiefly of importance to emphasize the consideration that the statement, "what does not interest will not be attended to," amounts to saying that whatsoever is not brought in some way into connexion with the previous experience or past life of the subject will not 'attract' or 'engross.' It is often maintained that there are certain presentations of so vivid, so impressive, so striking a character that they cannot fail to elicit recognition, that they force their way mechanically, as it were, into consciousness in spite of any subjective tendency to oppose them. This, however, is, by no means, unreservedly true, or, indeed, true at all, as thus expressed. One need not appeal to exceptional instances, such as that of Hegel writing the last pages of the *Phänomenologie* within hearing of the "thunders of Jena." Ample confirmation could be

¹ *Mind*, xi. 310.

furnished from every-day life of the fact that so far as strength or intensity can, in this reference, be measured or spoken of in an absolute sense, the strongest, the most vivid, presentation may not succeed in arousing attention, still less in becoming clear by virtue of its intensity. To be attended to, even such presentations must in some way be assimilated with contents that form, or have formed, part of the subject's experience; they can only be attended to when the conditions of the inner life do not prevent it. And these reservations rob the contention of any significance it might be supposed to possess as militating against the truth of the principle which I am emphasizing.

In the second place, another circumstance may be referred to that will likewise be of service. The range of attention, the kind of relations which can be simultaneously apprehended, will vary with the successive stages in the development of mind. And not only the range of attention. There will be variation also, although not capable of exact numerical determination, in the complexity of the facts that can, at any moment, be attended to¹. In the earlier stages of mental growth, attention can only be given to relatively simple facts. What can be held together in one and the same act of attention is, in these earlier stages, comparatively meagre in character, formed into one whole by relatively few and for the most part external links of connexion. In a mind of a maturer type, facts of far greater complexity will be simultaneously attended to, the field of simultaneous attention will be of far greater extent. So that here, again, and from another side, we are led to our former result that capability of being connected with the trains of ideas and feelings, or, more generally, with the experiences, of the conscious subject is an essential requisite of anything being attended to. Meanwhile we are considering the case not of attention which arises deliberately and through resolve, but of attention that occurs on account of the 'interesting' or 'attractive' qualities of what is presented in perception. A novel or striking occurrence, let us say, impels us to notice it. At first sight, that may seem to conflict with the principle we have been laying down. But the opposition is apparent only. The absolutely unknown does not interest us. That which is new will not

¹ It is worth remarking, perhaps, upon the totally misleading problem which has been set, in this connexion, for experimental solution,—that, namely, of determining how many objects can be attended to at once. What are we, in such a reference, to understand by *one* object? *Every* object is complex, comprising a multiplicity or manifold of features. It is *this* complexity that is the vital matter so far as the range of attention is concerned; the mere number of what are arbitrarily taken to be single objects is a very subordinate consideration.

be entirely new; to be recognised at all it will have to be connected in some way with what is already part of our experience. Such links of connexion may be small or large in number, and the number will depend to a considerable extent upon the stage of our mental development. If the number be small, the object will be comparatively obscure. A young child's astonishment at the sight of a locomotive will not, for example, secure clearness and distinctness of apprehension. The indispensable condition of clearness and distinctness, namely, rapid and easy assimilation with what past experience has prepared, would, in that case, be wanting.

To recapitulate,—along the path we have been following, we seem to be on the track of a perfectly general law of mind. An object may be apprehended with varying degrees of definiteness, distinctness, and clearness, and every increase of definiteness, distinctness, and clearness is equivalent to recognition of an additional distinguishable mark or characteristic in the object in question. The possibility of recognising such distinguishing marks is conditioned by the variety of ways in which the object can be brought into connexion with elements with which it can be compared, contrasted, and related. It is, in mature experience, a sufficiently familiar circumstance that the character of what is observed depends largely upon the sum of acquired ideas which the observer can bring to bear upon it, and in this circumstance is to be found the explanation of the main features of what we have called secondary attention. An object will become for us definite and precise, in so far as we have the means of instituting a comparison between what is immediately given and what we are already aware of as the accumulated product of previous experience.

§ 6. If the principle which has so far guided us be sufficient to account for the phenomena of attention hitherto considered, there is certainly a strong presumption that the special features that call for explanation in the higher form of attention, known as voluntary or deliberate, will exhibit themselves as following naturally from the varying circumstances that make their appearance in the course of mental growth and development. And I believe that, in truth, this is the conclusion which a careful examination of the facts does enable us to reach. The activity of discriminating, comparing, and relating works, as we have seen, through means of the material freshly offered being brought into connexion with material that has been already supplied to, and discriminated by, the apprehending subject. Obviously, then, the direction taken by this activity in the successive stages of its history

will be largely influenced by any important distinctions that may gradually disclose themselves among the material which we have described the subject as possessing. One such important distinction—it is not too much to say, *the* most important distinction—which thus comes gradually to recognition is that indicated by the terms self and not-self. By degrees in the development of intelligence there is effected a definitely recognised separation between the trains of thoughts, sentiments, feelings and sense-presentations which are more or less constant and habitual, and which thus come to be regarded as constituting the prevailing centre or background of individual personality, and the relatively transient presentations and apprehended contents which come and go, and which the subject learns to contrast with and to distinguish from the totality of the former. The contents of our knowledge or experience, or rather certain of them, tend more and more to wear the aspect of an inward possession, and to become the instrument, as it were, by which we apprehend the world of objective fact. So soon as this distinction has attained any prominence in consciousness, it must of necessity influence in a very decided manner the direction, as we may put it metaphorically, of attention. For it will then become possible for the subject to differentiate between the cases where attention comes about through a presented object being connected with the contents of representations or ideas that are not specially included in the consciousness of self, and the cases where the activity of comparing and relating is carried on through means of those ideas and feelings which are included.

The consciousness of self has by no means the same content in all phases of its development. At first that content would be comparatively poor and meagre in its features, and would be mainly dependent upon what later it never entirely loses, the mass of vague sense-experiences and feelings that arise from the vital processes of the body. Now the sense-experiences which arise as the concomitants of bodily movements are pre-eminently of the motor type—those presentations of tension or strain (*Spannungsempfindungen*) that are the ways in which muscular contraction is apprehended. Comparatively early in the evolution of animal forms, bodily movements begin to lose their originally chaotic and random character, and tend to fall into regular orders and groups. And the presentations of tension or strain are not, therefore, apprehended separately or in isolation; they form parts of a continuous series, which is experienced more or less as a whole, and, what is of no less importance for our present purpose, is revived or reproduced as a whole.

By way of illustration, reference may be made to our own experiences when engaged in overcoming resistance, say in lifting a heavy weight. Presentations of tension or strain come to us from all parts of the body, not merely from the muscles that are directly concerned. But these presentations are experienced as one complex mass, and it is easy to see how this complex mass comes to be looked upon as having a single and independent existence of its own, which is taken to be the cause of the movement instead of being, as it actually is, its concomitant and consequent.

The truth is that the conditions which are involved in the rising into consciousness of the distinction between self and the objects of the external world coincide for the most part with the conditions under which there comes gradually to be formed what may properly be described as the individual will of the apprehending subject. The conditions which are concerned in the formation of the will, in this sense, are, one may safely assert, very largely bodily movements, or rather the experiences which are the concomitants of bodily movements. Certainly the conscious subject's control over the movements of the body is an acquired control, and cannot be supposed to be, in any sense, an original acquisition of conscious life. If we assume the presence, at the outset, of rudimentary impulses as the primitive germs of conation, the temptation must be resisted of conceiving of the movements that follow from such impulses as in any way foreshadowed or prefigured in the impulses themselves. Looked at from the standpoint of an individual conscious subject, the connexion between specific presentations or re-presentations other than motor and specific bodily movements is an arbitrary connexion; the connexion was not, I take it, primordially revealed to consciousness through some innate mode of presentiment. Only through experience, by the aid of association, can the conscious subject have come to connect the primitive impulses with definite objects, and, as a consequence, the impulses have come to assume the more complicated form of desires. A desire, in the ordinary sense of the term, involves the representation in idea of what is desired, and the possibility of forming such a representation obviously depends upon there having been experienced a long series of prior presentations and the satisfactions which they afforded. Or, to put the matter otherwise, there is implied, in the state of desiring, some recognition of the distinction between the real and the merely imagined or ideal,—recognition, in other words, in however obscure a fashion, of a relatively independent order of facts over against which the conscious subject stands opposed. Before anything like a normal correlation came to be

established between desires and the means of realising them, there must have preceded endless experiences of executed movements which were not thus conditioned. Only through experience could a conscious subject obtain the data which enable the discrimination to be made between what simply occurs and what occurs in consequence of a representation on its part. Only through experience could there be formed in consciousness the representation of that which will yield satisfaction to the subject. Only through experience could such a representation become so welded together in consciousness with specific bodily movements as to secure its realisation. Both the control of movement and the realisation of an idea or representation depend, therefore, upon the establishment of empirical connexions between certain phases of the inner life and certain modes of the bodily organism.

Regular groups or series of movements come by degrees to be associated with specific re-presentations or ideas, so that on the occurrence in consciousness of the latter the former tend inevitably to be re-instated. The actual execution of the movement or the actual realisation of the idea, if the latter involves change in the external world, is wholly an affair of the bodily mechanism, and the details of that mechanism are completely hidden from the conscious subject. Though the action is inspired by conscious purpose, consciousness obtains no information about, nor does it direct, the intricate adjustments through which the action is carried into effect. We exercise voluntary control over our movements by dwelling on the object immediately before us in conjunction with the idea of what we desire to accomplish, and the bodily movement supervenes in accordance with natural law. In short, the realisation of what is represented in idea, as also the voluntary control of bodily movements, is itself, as a psychological fact, the result of attention; such realisation and control are only possible through the elements of a present situation being discriminated from and contrasted with the elements of a contemplated end or purpose.

Now, the very circumstance that attention, in the form denoted above by the term secondary attention, thus lies at the root of any control we can exercise over bodily movements points of itself to the source of those experiences of effort or of activity which are frequently so prominent in voluntary or deliberate attention. Careful analysis has shewn that in the experience of effort, even of the effort described as 'intellectual effort,' muscular factors of diverse and varied kinds are invariably brought into play, and that kinaesthetic presentations and re-presentations invariably form part of what is then experi-

enced¹. Furthermore, it is not difficult to understand how the feeling or sense of effort should come to wear the aspect of self-activity. For motor presentations as being dependent upon subjective impulses and as being likewise extremely uniform in character, as contrasted with the visual and other presentations to which they lead, naturally come to be connected with the consciousness of self in the closest and most definite way. But to conceive of such effort as self-activity in the sense of being itself the cause of the process of voluntary attention is, it seems to me, an error similar to that of supposing that the sensations of strain or tension experienced in lifting a heavy weight are themselves the causes of the bodily movement.

§ 7. That the self-conscious subject does exercise control over his bodily movements and over his trains of thought, it is far from my purpose to call in question. The mental activity, however, involved in such control is, I should maintain, the activity which is involved in apprehension, in various degrees, throughout, and which cannot legitimately be identified with 'effort' as an apprehended fact. Mental activity, surely, may be one thing, and the activity apprehended in 'strain' or 'effort' another, and one need not be thought to be denying the first because one fails to find it specially manifested in the second. According to the view I have been taking, every state of mind, whether the consciousness of effort be an ingredient in it or not, is a state of mental activity; that activity may be involved no less decidedly in a sense of ease than in a sense of strain. In listening to one of Wagner's operas or in following an abstruse argument, the mind may be intensely active and yet the consciousness of effort may be at a minimum. On the other hand, when effort is declared to be the "distinct consciousness of opposition between what we call self and muscular resistance" (Baldwin), one seeks in vain to understand how the muscles *can* offer opposition to the self. Further, as Dewey points out, muscular resistance, whatever else it may imply, must involve as a fact of consciousness motor presentations, and if the self is actually exerting force that too must find expression in motor presentations, so that the factors it is sought to dispense with would have, after all, to be admitted. In contradistinction to a theory of this sort, I have been trying to shew grounds for thinking that the distinction between non-voluntary and voluntary attention indicates in truth differences in the stages at which the process is viewed, and is explicable when account be taken of the conditions under which the mental life develops.

¹ See, for example, the experiments referred to by Dewey in his article on "The Psychology of Effort," *Philos. Rev.* vi. 43.

THE PSYCHOLOGY OF VISUAL MOTION.

BY HENRY J. WATT.

- I. *Criticism of Wohlgemuth's physiological theory.*
- II. *The introspective nature and affinities of the after-effect of seen movement.*
- III. *The correlation between the introspective features of the after-effect and those of the previous objective movement.*
- IV. *Wertheimer's criticism of certain psychological theories.*
- V. *The present theoretical outlook.*

Two elaborate studies of visual motion have recently been published by A. Wohlgemuth¹ and by Max Wertheimer². Both of these important papers add much to our knowledge of the facts, and excel in clearness and precision of work. They are also alike in rejecting all the psychological theories that have been advanced in their several fields of research and in formulating a physiological theory in explanation of the facts. Neither writer, however, makes any contribution to psychological theory. The possibility of such a thing is hardly even suggested; it is presumably annulled by the mere offer of a physiological theory. But the matter is not debated.

This situation seems to me so anomalous as to be worthy of special notice, the more so as the facts of the case hardly warrant the attitude adopted by these writers. In this paper I propose to deal briefly with the theory and outlook of these two works, which may be considered typical of a certain trend of opinion prevalent at the present time. In view of their general importance, however, and for the sake of brevity, I shall assume for the most part that the reader is already familiar with them and need only be reminded of their contents as each point arises.

¹ "On the After-effect of Seen Movement," this *Journal*, Monograph Supplement, No. 1.

² "Experimentelle Studien über das Sehen von Bewegungen" (Habilitationsschrift, Leipzig, 1912), *Ztschr. f. Psychol.* LXI. 161 ff.

I. CRITICISM OF WOHLGEMUTH'S PHYSIOLOGICAL THEORY.

The after-effect of seen movement, which is the object of Wohlgemuth's investigation, is familiar in various natural situations. If fixedly we gaze at a streaming waterfall or look down upon a rushing river for half a minute or so and then turn to look at the ground, the latter will seem to be streaming in a peculiar manner in the direction opposite to that in which the water flowed (relatively to our field of vision). For experimental purposes a simple form of this process is devised. A sheet of paper bearing alternately black and white lines of some little breadth is fixed upon a drum, which is rotated so that the lines move across the field of vision more or less slowly in a direction perpendicular to their length. Under suitable circumstances the lines will appear to move backwards when the motion of the drum is stopped. As the eye has been fixed and steady all the time, this peculiar after-motion cannot be due to any motion of the eye after the stopping of the drum, but must be taken as the after-effect of the preceding motion. Some theorists have therefore supposed it appeared because we were deluded by the previous objective movement into being accustomed to motion and therefore into expecting motion for a longer time than it was really there and thus into seeing what we expected. But this can easily be disproved by the application of incognitive methods, which prevent us from knowing from time to time what really happens. If the same after-effect follows whether the observer knows what is really happening or not, it cannot be the result of an illusion of judgment.

If the mind does not work at all to produce this after-effect, then apparently the only task for theory is to extend the accepted notions regarding the general physiology of neural processes so as to cover the facts; or to imagine a neural mechanism which will shew why motion is sometimes perceived where nothing really moves and why it then runs in a certain direction, opposite to that of the preceding movement. For his theory Wohlgemuth assumes that retinal elements $a1$ and $a2$, $b1$ and $b2$, are each connected with a "subcortical centre of movement," consisting of summation cells $A1$ and $A2$, $B1$ and $B2$, and also in pairs with a *Schaltzelle* $S1$ for the a 's and $S2$ for the b 's. $A1$, $A2$, and $S1$ are also connected with one another, as are $B1$, $B2$, and $S2$. Impulses are sent by $A1$ and $A2$ to the cortex, but this system of centres of movement is independent of other centres, *e.g.* those for brightness, colour, local sign, etc.

(a) Wohlgemuth assumes (pp. 99 ff.) that in the hypothetical centre of movement, owing to the part played by the *Schaltzelle S1*, a state of facilitation lasts in *A1*, so long as the objective movement stimulates the eye, but that as soon as this movement is stopped, the state of facilitation in *A1* is replaced by a state of fatigue in *A1*. By this means, during the objective movement, *A1* is more excited than *A2*, while during the after-stage *A2* is more excited than *A1*. The psychical counterparts of these relations of intensity are, for the former, movement having the direction *A1*—*A2*, for the latter, movement in the opposite direction.

Now *this assumption posits the unfailing occurrence of so special a case that it seems to me to vitiate the whole theory.* We should rather expect many possible relations between facilitation and fatigue: facilitation frequently still increasing with psychical counterpart of similarly directed movement and after-effect, occasional balance with no visible movement or after-effect, frequent fatigue after longer stimulation with a reversal of both seen movement and after-effect, and thereafter periodic return to a state of balance. But, as we read on page 85, "no after-image of the sectors moving in the same direction as the objective movement could at any time be detected."

(b) *The theory offered virtually begs the question.* For, in order to suppose that the physiological basis of the experience of pure motion exemplified by the after-effect is a difference of excitation amongst the cells *A1*, *A2*, etc., and that the physiological basis of the direction of the felt motion is the spatial distribution of this difference of excitation amongst the cells *A1*, *A2*, etc., it must assume that the cells *A1*, *A2*, etc., already function as the physiological basis of different localisations, and that real directions within the complex of cells *A1*, *A2*, etc. (with or without actual physiological connexions between these cells) form the basis of felt directions; or it must assume that the cells *A1*, *A2*, etc., individually and as a complex, are connected and correlated with those other centres that are the physiological basis of localisations and directions. In either case the theory takes the physiological basis of localisation and direction for granted and only offers a theory of motion, treating it as a sort of intensive state, which refers and is attached to these localisations and directions, and endows the "sukzessive Aufspringen eines gleichartigen Eindrucks an verschiedenen Orten"—which we might perceive merely as such, were we beings devoid of the peculiar experience of motion—with this

unitary quality of continuity, namely "ein Hindurchgehen durch die zwischenliegenden Räume!"

But can motion really be treated in this way? Is it not rather the case that motion has a direction of its own, which may coincide with, or be opposed to, some other direction of which we are conscious apart from any motion? Is not also the velocity of a motion a characteristic of its own? Are not the motion, the direction, and the velocity, of motion—whether it correspond to a real motion or be pure motion in the sense of the after-effect—the essential aspects of this experience, its vividness being necessary in some degree, but as such relatively unimportant? The vividness of the experience may be to some extent interchangeable with its velocity, in so far as an increase in velocity is accompanied by an increase in vividness; but surely it would be contrary to experience to allow this vividness to usurp the place of the velocity of the motion itself, not to speak of its direction. And if motion presents a continuity that is not given in, or derivable from, the data of our space and time *Anschaunngen*, should we not expect to find an explanation of this continuity included in the physiological theory of motion? But it is evident that this continuity is taken for granted in the theory as stated.

It seems then that the theory in question offers an explanation really only of the vividness of the experience; and if against this must be written the arbitrary assumption which I have stated under (a), the balance leaves nothing to the credit of the theory. We must discover first of all what is the neural basis of pure motion, its direction, and its velocity; it will hardly be very difficult thereafter to find a basis for its vividness.

On page 19 Wohlgemuth says that Borschke and Heschel's admit that, as seen, the movement of two sets of straight rods at right angles to one another "can only be regarded as squares, moving in an oblique direction." This movement can, of course, be described as one pleases; but if it is felt as the movement of squares in an oblique direction, that must be due to psychical, or shall we say, central, reasons; for it is essentially the *Gestalt* of the square which determines the apparent movement. If a point on one of the rods were marked out by colour or shape, we should at once in so far be free from this apparent oblique motion of the squares². In connexion with this the

¹ Cf. Ebbinghaus, as quoted by Wohlgemuth, *op. cit.*, p. 108.

² Cf. Pleikart Stumpf, "Ueber die Abhängigkeit der visuellen Bewegungsempfindung und ihres negativen Nachbildes von den Reizvorgängen auf der Netzhaut." *Ztschr. f.*

forced explanation given of the results of experiment 28 on page 107 should be consulted.

(c) *The theory constitutes, as it stands, a lapse from the presumable parallelism of mind and body*; it fails to shew that the relations of mind and body, whatever they may be, follow any general scheme or plan; in fact, it suggests that they vary arbitrarily from one experience to another. For all would agree, I think, that the neural basis of the arrangement of the simplest sensory experiences in respect of their adherent localisations is, proximately or ultimately, the arrangement of neural units of some kind. Of course, we should not expect to be aware of the experiences correlated with these neural units, nor of their localisations, apart from some degree of excitation in these neural units. But neither should we expect to find that the essential aspect of their stimulation, with which alone experience is correlated, is the difference of excitation in them. For even if difference of degree of excitation were a necessary feature of the neural basis of the experience of motion, and of its direction and velocity, these experiences must first and foremost be correlated with the arrangements and interconnexions of the neural units and only secondarily with their difference of excitation. Difference of excitation would, then, be only a means of bringing different localisations with different clearness and insistency to the mind.

Thus we might revert to the simple theory of common sense and expect motion to be based upon the successive stimulation of neural units correlated with different positions. And it is to be noted that we have as yet no evidence that bears against this view or shews that the effect of motion is producible from simultaneously stimulated neural units, be they stimulated equally or differently. The facts of the after-effect of seen movement do not, of course, afford this evidence. They offer no other evidence than do the ordinary facts of motion. It is only in the eyes of such a theory as Wohlgemuth's that the stimulation of the neural units subserving motion is simultaneous and different. [When the stimulation of the elements of a neural complex in different degrees is said to be simultaneous, that means, of course, for Wohlgemuth as for others, simultaneous and continuous over a short stretch of time.] But he extends this explanation not only to the

Psychol. LIX. 324: "Im Vorbild stimmte die gesehene Bewegung nur nicht dann mit der berechneten Richtung überein, wenn irgendwelche Anhaltspunkte andere Auffassungen begünstigten," etc. Compare the effect of using broken lines and spirals, where the seen movement always corresponds to the objective movement.

after-effect of motion, but to ordinary visual motion. His hypothesis thus stands in sharp contradiction to the facts upon which it ultimately rests, that is, both to the facts of experience and to the facts known regarding the elements of the peripheral stimulation and their relative qualities, intensities, positions, and times. The positions of these elements differ in different times, so that in the several neural units stimulated by them, at least in those proximate to the stimulation, there must necessarily be successive differences, be they differences of intensity or of quality or of both together. Is it not, then, most reasonable to suppose that whenever motion is given, these successive differences occur throughout all the elements of its neural basis, be they proximate or remote?

Therefore it seems that Wohlgemuth's physiological theory of motion fails to shew that the relations of mind and body follow any general scheme or plan; or if it does so implicitly, it places a false emphasis on the part played by the intensive differences of neural processes in the correlation of mind and body.

II. THE INTROSPECTIVE NATURE AND AFFINITIES OF THE AFTER-EFFECT OF SEEN MOVEMENT.

The first task of psychology seems to me to be a thorough study of all distinguishable varieties of experience and their arrangement on the basis of their resemblance to one another, whether the resemblance be that of appearance (*e.g.* of attributes) or of functional properties and variations. We must form a periodic table of experiences, as it were, and we must take that table as the basis and object of explanation of every theory which is to be called psychological.

From Wohlgemuth's valuable historical and experimental researches it appears that the after-effect of seen movement has the following characteristics or properties¹:—

- C* 1 The after-effect is an apparent movement, in a direction opposite to that of the previous objective movement.
- E* 16 Its velocity is comparable with that of an objective movement.
- E* 15 Its velocity acts as a velocity. It adds itself to an objective movement.

¹ *C* refers to conclusions by agreement between Wohlgemuth and his predecessors, *E* refers to Wohlgemuth's own experiments (the numbers are those of his text), cf. pp. 110 ff.

- E* 33 Certain observers mistake it for a real objective movement and are unwilling to believe the contrary (p. 87).
E 10—13 It varies in vividness (cf. pp. 46 ff.).
C 3 It is definitely localised.
C 4 It has a definite position in time.

These are its positive features. Negatively it appears that:—

- E* 32 It is not like "a shadow passing across the stationary surface."
E 33 As compared with an objective movement it has a hollow ghost-like appearance. Or it may have all degrees from reality to evanescence and ghostliness. As Wohlgemuth observes it, it is an experience *sui generis*. For him it never approaches the appearance of real objective movement. It lacks the solidity and reality that is given by change of position in space (cf. pp. 87 f.).

We must, therefore, conclude that, no matter how unusual the isolation of pure motion in the after-effect may be, nor how "unreal" it looks, it does greatly resemble its prototype of objective movement, fusing with the latter both phenomenally and functionally.

III. THE CORRELATION BETWEEN THE INTROSPECTIVE FEATURES OF THE AFTER-EFFECT AND THOSE OF THE PREVIOUS OBJECTIVE MOVEMENT.

The next question is whether the introspective kinship thus established is confirmed or contradicted by the evidence regarding the correlation between the introspective features of the after-effect and those of the previous objective movement upon which it is dependent. With which feature or features of simple sensation is the after-effect objectively connected? We may pass in review the chief attributes of sensation: (*a*) quality, (*b*) intensity, (*c*) order (local sign), (*d*) position in time, (*e*) extensity, and (*f*) duration. Of these, however, only the first four really come into question.

(*a*) *Quality*. The relevant facts are these:—

- E* 17—18 "The after-effect is independent of the quality of the light." The latter may be varied without variation of the former. Cf. *E* 19 below.
E 29 "Fatigue produced by alternating movements of opposite sign is independent of the colour of the light producing it, *i.e.* the fatigue is maintained in light of different colour."

E1, *C11*, and *E2—4* may also be cited, which shew the manner in which the clearness and vigour of the contents of the visual field reinforce the vividness of the after-effect. The after-effect is also noticeable in the dim field of subjective vision (eyes closed).

The conclusion, then, must be that the after-effect cannot well be produced apart from quality of some kind, but it is independent of the variation of the quality, as such, of the light. It is presumably produced by a factor which accompanies quality and which becomes, to some extent, more insistent as quality becomes more insistent. The explanation which Wohlgemuth offers of *E17—18* that "each new colour is a new stimulus" (p. 106) hardly seems consistent with *E29*. Wohlgemuth's theory may explain the latter, but it can hardly explain the former. Wohlgemuth himself seems to feel this difficulty (cf. pp. 107 and 109).

(b) *Intensity.*

- E19* "In the case of different colours difference of brightness is not essential for the production of the after-effect."
- E14* "If a moving series of alternating dark and light stripes excite the retina, a slightly better after-effect seems to be obtained if the stripes be of equal width; but if the alternate dark and light stripes be not of equal width it seems not to matter which stripes are increased and which decreased in width."
- E2—4* "The after-effect is more marked in a brightly illuminated objective field...than in a darker field."
- E5—6* "If during the passage of images over the retina, a stimulus of a given intensity alternates with one of less intensity, the after-effect of movement produced is more vivid than if such stimulus alternates with a (more or less complete) cessation of stimulus."
- C1* "The uniform passage of light stimuli over the retina in any given direction...produces the after-effect."

The decisive case is *E19*, which shews that a variation of intensity is not an essential condition. *E14* is only compatible with Wohlgemuth's theory if the special assumption discussed under *Ia* is admitted. The other results, along with those referring to the difference between the light- and the dark-adapted eyes, are concomitant variations, which may depend not only upon the variations of intensity, but upon that of one of the other attributes. The

explanation of *E* 5—6 which Wohlgemuth gives (p. 104) seems strained: "When a black stripe succeeds a white one the synapses, which had been fatigued, immediately regain their former state." But the main theory supposed these synapses to be in a state of facilitation. If they are fatigued, *A* 1 should be more fatigued than *A* 2, having been excited more strongly longer, and the movement should have turned apparently to the direction opposite to that which it shewed at first.

The conclusion then must be that the after-effect cannot well be produced apart from intensity of some kind, but that it is independent of the intensity, as such, of the light. It is presumably produced by a factor which accompanies intensity and becomes to some extent more insistent as intensity becomes greater. If the after-effect is to be got, moreover, either the quality or the intensity must be varied. Both of these may, but need not, be varied at once. The after-effect, therefore, cannot well be dependent upon either of these attributes, but it may be dependent upon a factor which changes with differences in either or both of these¹.

(c) *Unicocular order* (local sign).

C 1 Quoted above.

C 2 "This after-effect is more marked if the eyes...remain fixed on a stationary point."

C 8 "The after-effect is producible by any rate of the stimulating movement."

¹ It is necessary to refer at this point to the preliminary notice of experimental results issued by Pleikart Stumpf, in which he says: "Es zeigte sich nämlich zunächst die auffallende Tatsache, dass bei sukzessiver Helligkeitsänderung einer Farbe des einen Farbenpaares sich eine Stelle finden lässt, bei der der Bewegungseindruck in den meisten Fällen vollkommen verschwindet, oder in einigen besonderen Fällen doch ein Minimum an Deutlichkeit erreicht. Zu jeder Farbe lässt sich auf diese Weise ein bestimmtes Grau finden, das mit ihr, so müssen wir wohl annehmen, einen unwirksamen Erregungsübergang bildet, so dass kein Bewegungsempfindungsprozess mehr zustande kommen kann" (*op. cit.*, 328 f.). The grey is that which gives the lowest fusional frequency with the colour concerned. If Stumpf's observations are correct, their inconsistency with those of Wohlgemuth may be the result of the difference of method adopted. Stumpf's method is essentially stroboscopic and his bands of colour are very narrow—two millimetres. No account is given, however, of the means of obtaining the necessary variation of brightness in the grey bands, which to give the result stated must have been most laborious. Until full details are given, Stumpf's result must be held in suspense. In view (1) of the restriction of a "minimum in some cases" which he indicates, (2) of the absence of any reference by Stumpf to differences of velocity, and (3) of the cumulative effect of certain differences in Wohlgemuth's results, Stumpf's case must be supposed to be an exceptional one. At all events, the theoretical procedure upon which alone I wish to insist here, must be applied to all relevant and stable experimental results.

- C* 10 "Pseudo-movements, *e.g.* stroboscopic movements, produce an after-effect exactly as an actual movement does."
- C* 7—9 "The after-effect increases in one or several ways, within limits, with the number of stimuli simultaneously affecting a given area of the retina, and or with the frequency with which the stimuli pass given retinal elements."
- E* 10—13 "The after-effect at first increases very rapidly with the objective velocity, but soon reaches a maximum and then gradually diminishes with further increase of speed."
- E* 14 Quoted above.
- E* 21, 1—4 In the periphery of the field of vision the after-effect is at first more vigorous, but diminishes and disappears very rapidly.
- E* 21, 5 "Any after-effect in a not-stimulated area is of opposite direction to that of the stimulated area." [Not weaker or less rapid.]
- E* 5—6, 1 Here Wohlgemuth says that "distinctness of contours is not the essential factor in the production of the after-effect." But it is evident from page 37 that "distinctness of contours" is only an alternative reading for "difference of brightness."
- E* 28 "After fatigue has been produced by a long series of movements alternating in sign (so that the after-effect is greatly reduced), the after-effect of movements at right angles to the direction of the previous ones is only very slightly affected, if at all."
- E* 26 "When several objective movements of different directions stimulate the same retinal area simultaneously or successively, an after-effect is produced which is the resultant of the after-effects of the various movements."

C 1 obviously admits the influence of order and *C* 2 provides a better basis for its regular introduction. Contour is the chief form of accentuation of visual position, so that the greater the number and frequency of the moving contours the greater the variation of orders (*E* 7—9). The impression of motion comes into full effectiveness more or less suddenly after a certain rate of motion has been obtained, but it becomes less clear with the higher velocities (*E* 8, *E* 10—13). *E* 14 calls for the operation of a factor which is independent of the division of the period between the light and dark portions. It is, on the other

hand, a well-known fact that orders and distances are clearer when they are regular and symmetrical. *E* 5—6, 2, which shews that a grey stripe is more effective in alternation with a white one than a black one is, may be supposed to involve a greater clearness of orders. For when black and white are juxtaposed, they must intensify each other by contrast, and so make irregularities of brightness of their surfaces less noticeable than they would be if the black were replaced by grey. That is to say, grey favours the distinction of positions, or, in other words, it allows of the existence of many orders, besides that given in the contours. With *E* 26 we may compare what was said above about the apparent movement of squares in an oblique direction, when two sets of parallel rods move at right angles to one another. If the after-effect is correlated with the neural basis of orders, directions, and motions, there is no reason why fatigue for one direction should affect the receptivity towards another direction at right angles to the first (*E* 28). Wohlgemuth's explanation of this result, on the contrary, must be said to be highly strained (*vide* p. 107). As regards *E* 21, 1—4, it is a commonly accepted fact that motion is more insistent in the periphery of the field of vision, but that positions there are not so highly differentiated as in the centre of the field. We might, then, expect a more insistent after-effect of briefer duration, rapidly disappearing. It is difficult to see what relation *C* 10, especially as described by Wertheimer, has to the varying intensity of pairs of movement centres. But their relation to differences of order and of time is obvious.

The conclusion must, therefore, be that the after-effect is correlated with, and directly or indirectly dependent upon, the order-differences of sensation given by the objective movement which excites the after-effect. There is no fact which suggests that the after-effect is independent of this attribute of simple visual sensation. *E* 1 and *E* 2—4 only imply that the presence of clear qualities and high intensities involves clearer sensational orders than does a darker or obscurer field.

Of *C* 9 Wohlgemuth says: "This result is probably merely a question of fusion of two retinal fields like results Nos. [*C*] 6 and 7" (p. 103). These binocular cases do surely belong to quite a different class of integrative processes to be studied separately from unocular cases.

(*d*) The only other attribute which could come into question at all is that of *position in time* which represents rate of succession of stimuli.

It is undoubtedly involved in the production of motion and its after-effect, both in Wohlgemuth's theory and in any other. It is definitely involved in *C* 1, *C* 8, *C* 10, *E* 7—9, *E* 10—13, and *E* 14. Neither motion nor its after-effect is to be correlated with simultaneous sensations.

The following results do not apparently favour or disfavour any particular theory of the psychological or neural basis of motion and its after-effect: *C* 5, *E* 22, *E* 25, *E* 27, *E* 30.

The preceding investigation thus bears out the suggestions given by the psychical affinities of the after-effect of motion. This not only resembles motion, but it is related by direct psychical correlation with the experience of motion evoked by preceding objective movement, and with the conditions which favour or indicate a greater clearness of the orders of the sensations aroused by the objective stimulus. I offer no physiological theory alternative to that of Wohlgemuth. Nor do I mean to suggest that the after-effect of seen motion is linked to the preceding objective movement by any bonds of psychical causation. But I would maintain that the introspective nature of the after-effect is such that it resembles motion and order, while the correlations which experiment has established between the objective motion and the after-effect are such as to lead one to believe that the physiological basis of the after-effect is identical with that of motion and that both are connected with, and dependent upon, the physiological basis, not of intensity, but of order. A purely psychological statement of the resemblances and correlations between experiences must precede, not only every psychological theory regarding their connexion, but also, and *a fortiori* every physiological theory of their basis. It cannot be a safe proceeding to construct physiological theory by inference from psychological facts while the task of systematization of the psychological facts is neglected, whether a psychological theory of these facts is given or not.

IV. WERTHEIMER'S CRITICISMS OF CERTAIN PSYCHOLOGICAL THEORIES.

In Wertheimer's experiments, as in Wohlgemuth's, motion is seen when there is no real motion at all, but only the successive appearance, at times separated by varying intervals, of (usually) two brief stationary visual stimuli, *a* and *b*, separated by a short space, or at right angles to one another, like the two parts of the letter *L*. The

motion seen may be indistinguishable from the seen motion of a single real object, *e.g.* a short line turning through a right angle, or it may be double, as if first one small line made a movement through say 30° downwards from the vertical and then another small line through say 30° into the horizontal position; or it may even sometimes be so evanescent as to appear, apart from the motion of either small line, as a sort of pure, abstracted motion in a definite part of the field between the two lines, a mere 'going over' or torsion. All this, moreover, withstands the test of incognitive methods, just as does the examination of pictures shewn by the cinematograph. One may know the theory of the cinematograph or not, it makes no difference. So here again, the mind does not play a part in the production of the motion, not even by associating the parts omitted by the cinematograph with the parts shewn by it. For the motion will be seen even for objects that have never been actually seen by the spectator, *e.g.* an aeroplane, just as well as for the most familiar objects. Besides how could reproduction of the lost stages make the pictures move when they are not in motion at all, so long as they can be apprehended by vision as pictures? Thus we seem again to be driven to the physiology of the central nervous system for an explanation.

No objection need be raised against Wertheimer's physiological theory from the psychological side. Evidently it is only a theory of this kind which, as Wertheimer shews, can explain the facts relating to the production of motion by the stroboscope, the cinematograph, or other similar devices. I wish only to call attention to his criticism of those theories which attempt to regard motion as a form-quality (*Gestaltqualität*) or as a complex quality (*Complexqualität*) or the like, and which attempt to construct a psychological theory of motion from this leading idea. In Wertheimer's view these theories are put out of court by the fact that they demand that the motion which arises when the stimulations *a* and *b* are given in the manner described, shall apply to, and embrace, phenomenally both *a* and *b*. But, as Wertheimer has shewn experimentally, there are such things as "partial movements,"—*a* moving over one space and *b* moving over another space, the two movements being separated from one another by a small space; there is also such a thing as singular movement, when only *a* or *b* moves; and, best of all, the seen motion may not apply to, or embrace, *a* or *b* at all: these may be completely at rest and there may be in the space between them the phenomenon of pure motion or torsion, an experience much like Wohlgemuth's after-effect. Thus a theory which

suggests that motion is founded upon at least two contents, in this case *a* and *b*, may be dismissed without further comment. Besides such a theory would have to explain all the other facts gathered by Wertheimer, which, needless to say, it could never do¹. Wertheimer, finally, offers a physiological theory of the facts, his theory of "physiological short-circuit."

It may very well be that this or that theory of the type criticized has, in its ignorance of the facts, attempted to explain what was known of the facts of stroboscopic movement by using *a* and *b* as "founding contents" (*fundierende Inhalte*). But a critic may be expected to see the virtue, as well as the vice, of a theory. Like the eastern monarch who was invited to witness a horse race, and replied: "I already know that one horse can run faster than another," may we not also say: "we know already that there can be two disconnected movements, or that one thing may move and another be at rest, or that a motion may take place in the space between two things without affecting either?" Surely if Wertheimer offers a physiological theory of his facts, he thereby discredits his criticism of the form-quality type of theory! A felt motion may have any manner of cause you please, so long as the felt motion is supposed to correspond to its subservient, central neural basis. In Wertheimer's experiments *a* and *b* are mere stimuli, not founding contents.

V. THE PRESENT THEORETICAL OUTLOOK.

I indicated in the opening lines of this paper that neither Wohlgemuth nor Wertheimer explicitly discusses the general attitude he adopts towards psychological and physiological explanations. They do not say why a psychological theory need not be offered for certain facts, nor why a physiological theory of these facts is admissible. Probably the reader is supposed to be sufficiently disciplined in these matters already. But if the relations between the component parts of a complex attitude remain obscure, there is grave danger that one of these parts may be over-emphasized and overworked, so that confusion results. It will therefore be well to discuss this attitude, to clarify the relations of its parts, and to find which should dominate the others if the best and most harmonious results are to be obtained. *The whole situation may be seen analytically by means of a survey from two opposite points of inquiry.*

¹ Cf. Wertheimer, pp. 242 f.

(a) What reasons can be given for the absence of psychological theory?

There are three which may be imputed to these authors. Either (1) they feel convinced that there is nothing for such a theory to explain; or (2) they see in experience no basis upon which pure psychological theory might be built up; or (3) they are convinced that the facts of experience are mere discrete differences which can be explained only by physiological theory, based upon the special relations between experiences and the various features of the stimulative processes which evoke them. Acceptance of the third situation obviously excludes occupation of the first two. For if there is nothing to explain, there is no need for a theory of any kind; and if experiences are not connected in some way, but are mere discrete differences or qualities, the physiological entities (mechanisms, etc.) deduced therefrom will also be discrete and unconnected and therefore useless. And that is what we find; for just in so far as Wohlgemuth and Wertheimer identify pure motion or the after-effect with ordinary motion, they construct their physiological theories to accommodate both; and in so far as Wohlgemuth distinguishes motion from successive and continuous change of position¹, he must be held to give a purely illusory theory of motion or he assumes the existence of what he calls a "subcortical centre of movement"; and that, after all, is nothing but a ready made, specially created machine, which cannot have evolved out of the fundamental neural processes. But surely both the body and the mind must evolve; and if so each must evolve out of its own fundamental processes by the inner necessity and illumination that is given by progressively increasing effectiveness. To treat experience as a heterogeneous collection of elementary varieties, more or less similar, but essentially independent, therefore renders every scientific endeavour based upon the study of experience nugatory. Experience, like the starry sky which guides the sailor, is not merely one of the happy accidents of creation, merely "just so," and no more. It was a world of life before the sceptics tried to take it as an occasional, natural chart to the dark oceans of neural physiology. And it will be all the better a chart when it has again taken its place in knowledge as an ordered, inwardly coherent world.

If then we neglect the systematization and theoretical study of experience, we upset the natural hierarchy of the component parts in the complex task of the psychologist or psychophysicist and so achieve confusion. Neglect to systematize experience leads to neglect to systematize the physiological mechanisms we imagine by inference.

¹ Wohlgemuth, p. 88.

² *op. cit.* p. 99.

And without systematization there can be no theory of the evolution either of the brain or of experience.

(b) Adopting an opposite point of inquiry, we may now ask: *What insight justifies the confidence with which a physiological theory of certain facts is offered and admitted?* When double contacts give single touch, anyone apparently may understand that that is explicable only by a theory which assumes the existence of a single point of maximum central excitation and explains the way in which that arises out of the given double peripheral excitations. When an after-effect of negative sign arises from preceding objective movement, or when a movement arises from one or more resting stimulations, anyone may likewise understand that no laws of mind lie hidden here. The assumption of an indubitable parallelism of mind and body seems to be the only justification of these views. But this assumption, as we have seen, is abandoned by Wohlgemuth in his special physiological theory of motion and its after-effect. Why should we, then, retain it at all? Why not maintain that, when single touch results from double contacts, both the central and the peripheral excitations are double, and that single touch is due to the fact that for the two excitations the soul has rendered only one experience? Alternatively we might assume that the two excitations really did arouse two sensations, but that these two fused for some reason into one. Such assumptions have indeed been made, not perhaps for double contacts, but for those binocular stimulations which result in single fused vision. And no charge of absurdity or of obvious error could be brought against them. But these two cases of single touch and single vision from double stimulations are essentially parallel in nature. For the former only physiological explanations are generally admitted; for the latter physiological reasons have also been given, but they have been held to be utterly inadequate and psychological interpretations have been favoured instead¹. If the physiological or the psychological line of explanation is preferable in special cases, there must surely be clear ground for the preference.

This ground seems to me to be a tacit recognition of the possibility and validity of pure psychological theory. In dealing with single touch or any other similar sensations, we recognise that there is nothing psychologically simpler and more primitive than elementary sensation itself to which we might appeal for an explanation of its characteristics. Consequently, if one class of sensations shews features which another does not possess, we feel justified in assuming that the anomaly must

¹ Cf. W. McDougall, *Body and Mind*, London, 1911, chap. xxi.

be due to the peculiar nature of the stimulus or of the receptor of that sense, *i.e.* it must be due to physical or physiological causes. All sensations, then, must be of one psychological class and of one psychical type and must behave, apart from extraneous causes, in the same way. This assumption is quite admissible as a working hypothesis, since no positive arguments can be brought against it, no matter how difficult it may be to establish it. On the other hand, the admission that, apart from discrete differences in quality and in the extent of range of variation of any attribute, sensations may be of different types, is scientifically self-destructive. For the departure from type means the failure of generalisation and therefore the absence of explanation. There can be no true science of psychology at all, unless the simplest sensations conform essentially to one type. Hence the common appeal from the psychology of the sensations to physiological theory implies both the admission of the assumption of types and the recognition of a fragment of pure psychology.

Similarly it is justifiable to offer a physiological theory of the after-effect of seen movement and of stroboscopic movement; for there seems to be no obvious psychical reason why the after-effect should be of a direction opposite to that of the preceding objective movement. If previous writers have offered psychological theories, a closer examination of the facts shews that the processes they appealed to are not involved in these experiences¹. Nor is there any apparent psychical reason why the presentation of a successive series of stimuli differing in position should arouse the experience of a continuous movement over a distance or of many small neighbouring movements, etc. Besides, these peculiar effects are so like the experiences evoked by objective movements that we may at once assume that the physiological basis of the latter is identical with that of the former. Psychological theory has, then, only to classify and systematize the varieties of movement experience and to set them into relations of resemblance to the already classified simple sensations. The result of this task defines the problem for the physiological theory of motion, which has not only to imagine a neural basis of motion, but has also to shew how it is connected with the neural basis of the simple sensations, besides indicating, by reference to the incidental features of the physical processes taking place in these neural structures, how the anomalies of the correlation of external or preceding stimuli and consequent experiences (reversed after-effect, movement from stationary stimuli) are to be accounted for.

¹ Cf. Wohlgemuth, pp. 90 f. ; Wertheimer, pp. 240 f.

This attitude towards the problems of simple sensations and the simplest other sensory experiences is confirmed by a consideration of those cases in which two systems of sense-organs, eyes or ears, work together to make certain experiences possible. A careful survey of the problems is here made inevitable, because the facts suggest the view that the unity of binocular vision has no unitary neural counterpart¹. We seem compelled to allow that we get unitary vision not only from double peripheral, but also from double central excitations. That the method of approaching these cases must also give *first place to positive psychological classification and theory* I have attempted to shew elsewhere².

¹ Cf. McDougall, *loc. cit.*

² "The Relation of Mind and Body," this *Journal*, 1912, v. 299 ff.

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THE COMPARATIVE METHOD IN PSYCHOLOGY¹.

By CARVETH READ.

- § 1. *Rise of Comparative Science.*
- § 2. *The Comparative Method assumes continuity of descent by heredity or tradition.*
- § 3. *Its explanatory force consists in shewing what this line of descent probably was.*
- § 4. *Breadth of the field of evidence ; as depending on the psychological assumptions involved.*
- § 5. *The causes of whatever modifications of any faculty may have occurred in the course of its descent must also be assigned.*
- § 6. *Comparative Psychology requires the construction of Animal Psychology.*
- § 7. *The difficulties of Animal Psychology.*

§ 1. Comparative Psychology is merely Psychology treated by the Comparative Method ; or the application of the Comparative Method to the study of mental phenomena, or to the interpretation of behaviour ; so far as the results of Zoology and Ethnology require for their complete understanding an appreciation of the mental processes of other men and animals ; and so far as such appreciation enables us to understand each type of mind by comparison with others, and thereby the better to understand our own.

The comprehensive idea of comparative science is modern ; we are now familiar with such terms as Comparative Anatomy and Comparative Philology. But sporadic attempts to throw a light upon some kind of fact by comparing different examples of it are old enough. Some of the sophists, after comparing the varying laws and customs of nations, concluded that there was no such thing as natural justice. Every empirical induction, of course, depends upon a comparison of

¹ Founded on a lecture delivered at University College, London, October 9, 1912.

cases. All attempts at dividing, defining, classifying, involve the making of comparisons: the great ancient example of this is Aristotle's classification of animals. In Psychology, Aristotle distinguished four grades in the activities of souls: growth and nutrition (common to plants and animals), perception, memory and reason; each with its cast of impulse or desire. In modern philosophy we find Descartes and Locke confidently comparing the minds of men and brutes. But all this is little else than classification: classification depends upon the making of comparisons; but the mere making of comparisons is not the Comparative Method.

The comprehensive idea of comparative science is not older (I believe) than the latter part of the eighteenth century, and was first applied to Philology, when an acquaintance with Sanscrit began to spread amongst European scholars. Its similarity to European languages was perceived; and the idea arose of classifying languages according to their agreement or difference in vocabulary and in principles of word-formation and syntax, and of investigating especially the Indo-Germanic languages, as exhibiting derivation and differentiation from a common ground according to laws of change. Next came Comparative Anatomy, and awakened the greatest interest and wonder by shewing what homologies, or resemblances of structure, were to be discovered in abundance throughout the animal kingdom.

Yet Comparative Anatomy, though profoundly interesting, failed for some time to enter upon the true Comparative Method. It is very interesting to learn how like the wings of a bird are to the forelegs of a lizard and to the arms of a monkey; that the pineal gland, the seat of Descartes' soul, is the same organ that in *Sphenodon* and some other lizards gives rise to a vestigial eye that opens on the roof of the skull; that the bones of a gorilla differ from a man's only in their proportions: but what then? Hundreds of such comparisons and identifications leave us in a state of intoxicated wonder; and wonder, according to Aristotle, is the beginning of science; but, certainly, it is not the end. The accumulation of myriads of such facts constitutes the statement of a problem, but gives no solution, nor any method of finding one. It was not till the last century had passed its fifth decade that the key to this problem was supplied by the theory of evolution and of the genetic relationship of all organisms. And this is why Comparative Philology struck into the true method earlier; because the unity of the human race was already believed in; and so it was easy to understand that a primitive people, speaking one

language, might, after separating (under the Tower of Babel), and wandering far and wide, come to speak different dialects, and at last become mutually unintelligible. Hence the idea of tracing original relationship, by comparing extant or recorded tongues, and finding what they have in common; and, further, of tracing the history of each tongue as far as possible by documents, and (where these fail) by means of the corresponding words and forms of other tongues, and by the laws of their modification: this idea easily arose and became extraordinarily fruitful.

§ 2. The Comparative Method, then, always requires as its basis the assumption of continuity of descent, or of tradition, in the phenomena it deals with, and is applicable wherever such conditions are found. If continuity of descent is found in the development of the mind, the method is applicable to Psychology; and I purpose to draw the outlines of this method, and to illustrate its stages from Psychology and the Sciences with which Psychology is most closely implicated. Most of my illustrations concerning instinct may seem to belong to Zoology, rather than to Psychology; but that is unavoidable at present, since there has hitherto been so little successful work done upon the subjective side of instinct that purely psychological materials are wanting. In my opinion, moreover, there is no such thing as a science of pure Psychology; and since, if attempted, the data must be entirely introspective, it could only hold of the human mind:

The Comparative Method is not merely a drawing of comparisons, but of explanation by means of comparisons; and the explanation consists in reconstructing the antecedents of the phenomenon we are investigating and giving its history. Many people may have wondered at the English word "am," so conspicuous in our speech and so isolated in its appearance, and wonder may have convinced us of our ignorance (as Aristotle says it does); but most of us get no further. Philologists, however, on comparing other Indo-Germanic languages, have found that our substantive verb has three roots, *bhu* (to grow), *was* (to dwell), and *as* (to breathe); and that *am*, like the Latin *sum*, the Greek *εἰμὶ*, and the Sanskrit *asmi*, may be regarded as an abbreviation of a hypothetical Aryan *asuma*, where *ma* is the sign of the first person; so that the pleonasm "I am" has come to mean "I exist" from having originally meant "I breathe" with the implication of existence. It is (by the way) instructive to the Psychologist to learn that our substantive verb has been formed from three roots, none of which primarily expressed the abstract idea of existence which they all include: our

forefathers had the idea in an obscure and nascent form, before they had an appropriate word for it; and we still use all three signs, having forgotten their more concrete significance. Biologists and Psychologists proceed in the same way as Philologists, dealing with genera and species, instead of with languages and dialects, and trusting to find by a comparison of species, under the idea of common descent, the antecedents of structures and functions that occur in any of them whose history cannot be directly known.

Thus Darwin, in explaining the comb-building instinct of our hive-bees, which, taken by itself, is so marvellous, refers first to the humble-bee, which uses its old cocoons to store honey in, and adds to them some short tubes and irregularly rounded cells of wax. But the gap between the humble-bee's work and the finished architecture of the hive-bee is prodigious. However, he knows of a certain Mexican bee (*Melipona domestica*), whose comb (as well as her own bodily structure) lies about midway between those of the foregoing species; and he argues that certain easily-conceivable alterations in the habits of the Mexican bee, all favoured by the principle of economy or utility, would result in such structures as those from which we get our honey. Comparative Psychology must adopt a similar course.

Nothing, for example, is more conspicuous than the superiority of the human over the animal mind. Hence it has been felt to be a matter of first importance to shew that the higher mammalia, and especially the anthropoid apes, supply a sort of mean of intelligence and character between man and the lower mammalia: that the ape is as much above the monotreme as man is above the ape. And in Darwin's *Descent of Man*, chapters iii. and iv. consist chiefly of facts to prove that the most characteristic human faculties are foreshadowed in the higher mammalia and especially in the Primates; so that it is credible that the immediate ancestors of man, could we restore them, would present to us all the intermediate stages. So far as we may infer mental faculties from a skull, the remains of *Pithecanthropus*, found in Java, verify this position: the skull's capacity lying about midway between that of a chimpanzee and that of an Australian.

§ 3. The Comparative Method explains by pointing out a *possible* course of causation in the production of some phenomenon by tradition or inheritance; it does not show that such or such steps were actually taken, but only that (judging by parallel cases) their having been taken is more or less probable. If we could produce a record of the actual

steps, there would be no call for the Comparative Method: the recovery of actual combs of the hive-bee, shewing all its variations from the beginning, would make it quite needless to refer to the workmanship of allied species. The use of the Method is to construct or confirm an elaborate hypothesis concerning a series of antecedents that have been lost. Even in reconstructing the famous genealogy of the horse from an ancestor with five toes, the evidence (I understand) depends upon a number of specimens which make it highly probable that a certain series of changes in his structure took place, but do not supply the unbroken series of intermediate forms. Hence if one hears of only one or two cases of such reconstruction of genealogies, the evidence may seem feeble; but this feeling passes off, as more and more cases are all explained by the same method. When we are investigating some ancient instinct, or the rise of intelligence, we certainly shall not find the earlier stages of it preserved in the geological record; but we may find some of its correlative structures and some of its products, and may learn much concerning the climatic and biological environment in which it was exercised. Fossil evidence of the existence of gall-wasps and their galls is found throughout the tertiary strata: we are, therefore, sure that the correlative instincts of wasp and larva were then exercised. The history of an instinct, then, though the very steps of its descent to extant species be irrecoverable, may conceivably be reconstructed as well as that of the horse's anatomy.

Geology (I may observe by the way) seems remote from Psychology, until we see that, in the department of Palaeontology, it is very helpful and even necessary. It has been suggested (for example) that the relatively small brain of Dinosaurs, compared with mammals of equal bulk, may explain the extinction of their order: inasmuch as the small brain implies low intelligence, and incapacity to modify behaviour in the presence of changing conditions of life. If so, and if no other cause of their extinction can be shewn, we may turn the argument about, and rely upon the superior adaptability of intelligence compared with instinct, and its consequent biological utility, as one of the reasons (or the chief of them) why intelligence gives a predominance to those organisations that trust to it.

The course of causation that is pointed out by the Comparative Method comprises: (1) a probable line of descent, along which a certain organ, or instinct, or institution (in social affairs) has been handed down by inheritance or tradition, and has been gradually modified; and (2) the causes of each modification so far as they can

be assigned. An hypothesis thus constituted is, of course, subject to the usual logical conditions: it must be comprehensive, consistent with all the known facts, and better in these ways than any rival hypothesis.

As to the line of descent, Darwin says: "In searching for the gradations through which an organ in any species has been perfected, we ought to look exclusively to its lineal progenitors; but this is scarcely ever possible, and we are forced to look to other species and genera of the same group, that is to collateral descendants of the same parent form, in order to see what gradations are possible, and for the chance of some gradations having been transmitted in an unaltered or little altered condition. But the state of the same organ in distinct classes may incidentally throw light on the steps by which it has been perfected" (*Origin of Species*, c. vi.). Accordingly, in discussing the comb of the hive-bee, he confines himself (as we have seen) to collateral descendants of the same parent form; but when he explains the possibility that the vertebrate eye has been produced by natural selection, he refers to the faceted eyes of insects and crustaceans to illustrate the range of variation, and to primitive forms in which there is no lens, and even to aggregates of pigment-cells without any nerves. Now, for Psychology, the history of the eye indicates the history of the power of vision, so far as it depends upon the peripheral organ. Similarly, if the antecedents of the human mind are sought only in the Primates, the inquiry is confined to "collateral descendants of the same parent form"; but we seek to throw light upon our subject matter from "the state of the same organ in distinct classes," or to classes whose connexion with ourselves is more and more remote, when we turn for parallels to dogs and cats, or reptiles, or fishes, or even go to the ant and consider her ways.

§ 4. The field of evidence, then, from which parallel cases may be drawn seems to become of indefinite extent. No one has used the Comparative Method more powerfully than Dr Frazer in elucidating the origins of social beliefs and institutions; and to find evidence for his speculations he ransacks the whole storehouse of human records contemporary and historical. In *The Magic Art* (vol. II., cc. xiv. to xvii.), discussing the worship of Vesta at Rome, he derives from various sources some significant hints concerning similar practices among the Latin tribes and the Kelts and the Greeks, who stand to the Romans in the relation of varieties or proximate species, like the humble-bee and *M. domestica* to the hive-bee; but for the closest parallel to the institution at Rome he turns to the Hereros or Damaras of South-

Western Africa. One may ask how a case so remote can throw any light upon the argument. No doubt a parallel from Keltic or Greek religion would have been more acceptable: and that similar institutions at some time obtained amongst those peoples is highly probable; but no record remains of them, and therefore none can be adduced. Concerning the Hereros recent and credible testimony is forthcoming; and it is justifiable to resort to it for the following reasons. It can be shewn that the modes of inference and the other mental conditions determining belief, and the beliefs resulting from such conditions, amongst races at about the same level of culture, are very similar, and are more alike the more primitive they are. Their social conditions as to family and tribal organisation have much in common. The main external conditions of their life are also similar: dependence on the sun and weather, on the fertility of flocks and herds and fields (if they own or cultivate any), on the kindling of fires or the preserving of a perpetual fire. And causes being similar, so are the effects. Hence arise similar doctrines concerning the divine nature of fire, and institutions for kindling, maintaining and worshipping it. Plain signs, if not explicit evidence, of the existence at some time of such beliefs and practices can be found in Asia and America, as well as in Europe and Africa; and it is therefore not unreasonable to confirm an hypothesis concerning the customs of the legendary age of Rome by describing the customs of the Hereros. Indeed it has this advantage, that the more remote, racially and geographically, the peoples are one from another, the less likely are their resemblances in belief and ritual to be traceable in any way to imitation¹.

This argument of Dr Frazer's depends entirely upon the psychological position, that the modes of inference and other conditions

¹ On the other hand chronological remoteness of peoples (in the above case, about 2500 years) leaves more opportunity for possible transmission of influence by imitation, or even for tribal migration. No one will suppose that the Hereros have imitated, however indirectly, or are actually descended from, the early Romans; but Professor Sollas suggests that the Bushmans with their remarkable pictorial art are the same people who painted the walls of caves at Altamira and elsewhere on either side of the Pyrenees (say) 20,000 years ago. There is time for the migration to have taken place, and perhaps some indication of the route pursued may be given by painted rocks in various parts of Africa from Morocco to the Cape. Moreover there is some evidence in their stone-carvings that the artists of the Pyrenees were, like the Bushmans, steatopygous (*Ancient Hunters*, etc., c. 9). Herodotus doubting the claim of the Sigynnae, dwelling beyond the Danube, to be descended from the Medes, adds that, nevertheless, given a long time, nothing is impossible (B. v.). The art of the Bushmans differs in some characters from that of their supposed ancestors (and no wonder!); but if this hypothesis should be confirmed, we have here a remarkable proof of the persistence of racial traits.

determining belief and (through belief) action, are everywhere the same amongst men, so far as men are unscientific, and that therefore under similar conditions we shall find similar practices. The extensibility of this principle sets a limit to the Comparative Method. The cruder conditions of belief give way, in some circumstances (as under stress of trade and industry), and in some minds (least subject to the 'crowd'), to more exacting tests of truth, which culminate for us in scientific method. Few of us, indeed, apply this method outside of our special studies; but so far as any people do so, it is impossible for the practices connected with Magic and Animism to establish themselves; since such practices depend upon social beliefs concerning the connexions of phenomena accepted without analysis; which may, therefore, be groundless and absurd; whereas scientific investigation seeks the necessary relations of things.

Similarly for the whole Animal Kingdom there are certain indispensable assumptions which mark the limits of Comparative Psychology. The first requisite is the presence in all animals, even the simplest, of some degree of consciousness; the second is the connexion between sensation and reaction, and between reaction and feeling. At a higher level the associability of sensations must be assumed; higher still the associability of perceptions, and (again) of ideas. To determine the marks by which to judge whether a species lives at one or another level is a leading problem of our study. Not that the levels are sharply demarcated; but to assume in any case a principle of subjective interpretation which the given species does not share in, must lead to disappointing results. Where, in interpreting the behaviour of a given species, a principle requiring a certain stage of development is assumed, parallel cases can only be looked for at, or above, that stage. And if there is no fundamental similarity of mental processes throughout organic nature, it is impossible to reason freely from one case to another, or to trace by parallel reasoning any line of descent for the mental faculties. But it is the *laws* of mental functions that must be assumed everywhere to prevail: many particular functions may be dispensed with. There are secondary characters of mind, such as colour-sensibility, or vision as a whole, or hearing, or 'image,' as to which it is important to find out experimentally whether they are present or absent; but their absence does not disable our judgment of an animal's mind, such as it is.

And a similar observation may be made in Ethnopsychology; we may distinguish between fundamental and secondary characters of

culture, depending on fundamental and secondary traits of mind. Fundamental, for example, are magical and religious rites; secondary are art-products: the former depend upon universal modes of inference and conditions of belief; the latter may be due to several motives, varying in strength from tribe to tribe, and one or more of them possibly absent here or there—magic, religion, commemoration of events, communication with others, industrial value, or artistic delight in the work. Any art-product—a drawing, decoration or dance—being given to find its motive or motives, we are at first perplexed by the possibility of a “plurality of causes”; but this difficulty generally gives way upon examination of the details. On comparing as many examples as possible that are known to have chiefly some definite motive, marks may be found of one motive, others of another, and so on; so that, looking for such marks in the given object, we may assign it to one of the motives, or to some of them. It is true that an expert may form a respectable judgment upon such matters without being able to explain why; but if he could explain, it would be by bringing forward the distinctive marks of each class, which lie submerged and unanalysed in the total impressions of his experience. If the motive of any work of art cannot be assigned either scientifically or empirically, we are so far left without a clue to the psychology of the artist and his people.

§ 5. As to the causes of modifications that take place in the line of descent, they are of two kinds: (1) General advantages, reducible for the most part to ‘utility,’ such as protective resemblance, or economy—say economy of wax, which (Darwin thinks) must have favoured the survival and spread of bees that built the best comb; or adaptability to new conditions, which must promote the success of animals of superior intelligence. (2) Particular conditions, such as the Arctic glaciation, which (according to Wallace) may have originated the migratory instincts of many birds; or increase of predatory enemies, which would give survival value to alertness, quickness of perception, promptitude of action (in flight or defence) in herbivores, according to the disposition of each species.

If particular conditions are assigned for any change of structure, instinct, faculty or custom, there should be evidence, direct or indirect, (1) that the alleged conditions—say glaciation—really occurred at the time, and to the extent, required for producing and confirming the modification; and (2) that they might be expected to produce it, either for general physical reasons *a priori*, or because in parallel cases similar results have happened (that is, by the Comparative Method). The

glaciation of the whole Arctic region and a great area of the temperate zone, in the Pleistocene period, has been very generally admitted ; but the extent and continuity of it is disputed by Dr Scharff (*Origin and Distribution of Animal Life in America*) ; and the adequacy of such an event to produce migration is disputed by Mr Dixon (*Migration of Birds*), who urges that such a climatic change would bring about not the migration but the extinction of species. I mention these objections to Wallace's hypothesis by way of illustration, offering, of course, no opinion as to their validity.

In Psychology the principle of utility is frequently appealed to in order to account for successful modifications in conduct both in animals and in men. It explains (for example) the appearance of the parental instincts in a few fishes and reptiles and in most birds ; since the death-rate is reduced, fewer offspring become necessary to maintain the numbers of the species, and the development of individuality is favoured. The utility of numbers for mutual defence, or of giving the alarm when danger threatens, explains the gregariousness of most Ungulates ; the utility of numbers for attack explains the gregariousness of wolves and probably of our own remote ancestors. Sociality having been established, it becomes a particular condition of most other traits that distinguish social animals ; and the further development of society by internal differentiation of occupation and rank supplies at every stage particular conditions of nearly every peculiar belief, custom, and character. The establishment of the Kingship, for example, seems to be such a condition of the growth of a belief in gods who are kings, of the rites of their worship and of the feelings with which they are regarded.

The general utility of intelligence for the sake of adaptability is plain. As to the particular conditions of the development of intelligence in the Primates, including ourselves, Professor Elliott Smith lays great stress upon the habit of tree-life, characteristic not only of the lower Primates but also of their ancestors. The fact that they had this habit will not be disputed, nor the persistence of it during a sufficient period to have important consequences, since the remains of lemurs are found in the Eocene strata. The habit of living in trees diminished the predominance of the olfactory sense, and favoured the activity of the visual and auditory distance-senses, as well as the tactile sense and the kinaesthesia, and also the development of those areas of the brain that subserve attention and association¹. Perception of an enemy or of food,

¹ *Address to the Anthropol. Sect. of the Brit. Ass., 1912.*

by the distance-senses, sight or hearing, implies an interval of time, longer or shorter, before contact can result in injury from an enemy or in the obtaining of food: an interval that may be filled with many mental processes favourable to escape or possession, and therefore giving advantage to those individuals in whom such processes occur and in whom they are most effective¹. And perhaps I may be allowed to refer to the hypothesis set forth in *Natural and Social Morals* (c. vii. 2), that it was the adoption by man of a carnivorous diet, and of the habit of hunting in pack the larger kinds of game, that supplied the main condition for the early developments of human traits both physical and mental in our anthropoid forefathers. That vegetarian animals may adopt a carnivorous, and carnivorous a vegetarian diet, or a mixed one, is shewn by many examples: not only domestic dogs and cats, but also wild pigs, squirrels, bears, etc., and some monkeys are partially carnivorous, to the extent of eating birds' eggs and even young birds. The possibility of extensive changes of diet is implied in the hypothesis that all the higher mammalia are derived from an insectivorous stock. Again, the earliest men we know of were hunters, and their remains are found with those of large game. The advantage of hunting in pack is shown by the dogs and wolves; and it is possible that some of these joined our own pack at the beginning, and have shared our fortunes ever since. To the pack language was useful, and that gave the basis of nearly all our intellectual superiority.

Between the derivation of instincts and customs by heredity or tradition and their modification by general influences or particular conditions, there is no incompatibility; on the contrary, the latter presupposes the former. Dr W. H. R. Rivers, in his Address to the Anthropological Section of the British Association (1911), on the *Ethnological Analysis of Culture*², tells us that, having formerly been too much addicted to the explaining of customs and institutions everywhere by independent evolution, he became convinced by his experience in Oceania of the necessity of attributing many important social phenomena to racial mixture and the blending of customs. The proofs that he gives of this must convince everybody else, even if we had not many examples to the point in the history of our own country. But he does not mean (I believe) to disparage the idea of the independent evolution of customs. Customs must exist before they can be blended. And as

¹ Cf. Sherrington, *The Integrative Action of the Nervous System*, c. ix.

² Cf. Dr Rivers's paper, "The Sociological Significance of Myth," in *Folk Lore*, Sept. 1912.

for the Comparative Method, it is as applicable to the modification of customs or institutions, of instincts or intelligence, by migration, conquest or imitation, by changes of climate, habitat or food, as it is to descent by tradition or heredity. Where, for example, it may be impossible to find direct evidence of a conquest, such as one supposes to have changed the customs of a people, indirect evidence may perhaps be adduced that similar changes have resulted from conquest in other countries. If we look for similar cases in animal life, we shall hardly find that any species has altered its behaviour by imitation¹ of another species; but migration and conquest (exposure to new enemies) must often have extensive consequences; such as the reawakening of fear in the birds of a desert island, after man has come amongst them and disturbed their "ancient haunts of peace." And indirect evidence of a change of behaviour may be all that is available. There is no direct evidence (I believe) that the swallows that now build under our eaves formerly built elsewhere; but we cannot suppose that they first began to build nests when they recently found houses convenient to shelter them; and we know that an allied species, the house-martin, now builds in caverns and in cliffs as well as in houses, and that many a species of bird, spread over considerable areas, builds under different climatic or defensive conditions, different kinds of nest differently located. So that the modification of instinct in the house-swallow presents no difficulty.

In these cases, from the observation of certain modes of behaviour, we infer a change and the causes of it. Conversely, if there are grounds for believing that a certain change of conditions has had certain consequences, we may look for similar consequences wherever a similar change has taken place: if, for example, the development of vision and hearing, as distance-senses, is held, with good reason, to have led to the improvement of intelligent behaviour, it may be surmised that the earlier development of the olfactory sense contributed, in some measure, to the same sort of progress.

§ 6. If we are to apply the Comparative Method to the explanation of the human mind, we must allow ourselves great latitude of comparison. The human mind cannot, of course, be explained merely from itself. If we refer the behaviour of an adult Psychologist (whose mind is necessarily our starting point) to its beginnings in childhood, it soon appears that the child's mind is quite as much in need of explanation.

¹ I mean, of course, 'by *conscious* imitation'—to be distinguished from biological mimicry.

After a few months it exhibits numerous faculties, keen and adroit, out of all proportion to its experience; so that Romanes could rank the child of two and a half years on a level with the adult gorilla: though the comparison is deceptive; for in human affairs, in which he is at home, the child is plainly in advance of the gorilla; whilst in the gorilla-world, which we do not appreciate, he would be far behind. As the child develops year by year, we see that his powers are not merely an acquisition of his own life, whether by experience or by education in the widest sense, but the realisation of an inheritance; and this is clearly indicated by the disproportionate size of his brain, and the rate of its growth in infancy, relatively to the size of his body and to the rate of his brain's growth in later life. If we turn to less cultivated races than our own, or to what are called 'savages,' we find in all of them extensive knowledge (according to their needs), intelligence and dexterity, traditions and institutions, which make it quite hopeless to seek our own beginnings in them. Of the anthropoids, in their native state, or monkeys, or lemurs, we know too little, but we know enough to see the absurdity of looking for a beginning there. And the same thing is true of all vertebrates and of all the invertebrate metazoa. It is plain that organisation always implies antecedents, that orderly growth and development always imply heredity. Hence there is no alternative to the task of attempting to construct a comprehensive Animal Psychology. This is not itself Comparative Psychology, except in so far as it is constructed by the Comparative Method; but the study of animals by experiment and observation upon each species must supply a large part of the necessary data for the Comparative Method. Indeed, a good many Botanists will tell us that a thorough explanation of behaviour cannot be obtained without including Plant Psychology; since plants have their own organs of perception, and, when these organs are stimulated, messages pass by protoplasmic channels to other parts of the organism, and are responded to by appropriate movements, comparable to those which in animals we regard as signs of life and mind.

§ 7. The task of working out Animal Psychology will need very many years of labour and the co-operation of very many students. It is far more difficult than the earlier investigators were aware of; so that much of their work not only needs revision, but must often be treated as a warning against certain fallacies. It is more difficult than the Ethnologist's task; for we are sure that human institutions begin with man, and therefore we may hope to reconstruct the history of culture, both in its facts and in its motives, from our observations, and

from the records and remains of man in his various tribes and races; but we are equally sure that the subject of Psychology, sensibility and reaction, has no beginning but with life itself. On the other hand, a considerable part of the work, especially descriptive work on the senses and nervous systems of animals, and on their instincts, has already been done for us by Zoologists. Animal Psychology is a way of regarding a certain area of the zoological field, namely, the behaviour of animals, considered as susceptible of subjective interpretation; and I hope that few Zoologists would admit that it is separable from their own study. Zoology, Anthropology and Psychology are conveniently distinguished for the sake of special work; but, like the Siamese twins, they cannot be separated without sacrificing the lives of all of them.

The chief difficulty of Animal Psychology, however, is not its comprehensiveness and vastness; to overcome that would be merely a question of time. There is, besides, the intrinsic difficulty of finding a subjective interpretation of the facts. In dealing with men of other races, or of other levels of culture, we may be confident that the general ground of their mental constitution, their senses, impulses, emotions, and the laws of the formation of habit and belief, are very much like our own (at the pre-ratiocinative stage); so that, beginning with our own experience and proceeding cautiously and methodically, we may hope to understand theirs. With anthropoids, too, and monkeys we feel upon pretty sure ground; and even dogs, though belonging to another branch of the tree of life, yet, as social animals, lie open to sympathetic interpretation, and we are upon most points of behaviour upon terms of mutual understanding with them. But, as soon as we leave the human race, we lose the power of verifying inferences concerning the mental experience of others by obtaining from them direct introspective descriptions or replies to questions; and below the level of the higher mammalia our difficulties rapidly increase. We may say, slightly altering the words of Spinoza, that "the minds of animals differ from ours as their bodies do" (*Ethica*, III. 57); and, accordingly, the more their bodies differ from ours, the harder it is to understand their minds. Moreover the likeness or difference of bodies is no matter for a superficial judgment; it is concerned with intimate structures, recondite and perhaps still impenetrable. How much could we infer of the difference in character between the cat and the dog from anything we know of their bodies? Our children shew a delight in climbing trees, presumably by remote inheritance (which must have some unknown physical basis), although the characteristic changes that have taken

place in the human body all go to disqualify them for the Primate's ancient habitat. In some animals, again, there seem to be organs of sense in which we have no share, such as the lateral line in fishes; organs serving similar purposes to our own, but so differently constituted that the experience obtained through them must be very different, such as the multiple eyes of insects and crustaceans; and other organs whose significance we cannot even guess. Besides, since we do not perceive merely with the sense-organs, but with the brain, it is impossible to assume that animals with brains very different from ours perceive, as we do, even what their sense-organs, taken by themselves, seem able to perceive. For example, the eye of a glow-worm is capable of forming a complex image, which Exner saw and photographed; but it does not follow that the glow-worm's brain is capable of the considerable synthesis which the perception of such an image requires; to say nothing of its interpretation by ideas. There are the still more puzzling instincts in which many animals seem so wise concerning matters about which it is impossible they should know anything either by experience or by instruction.

With all such problems, to take the easiest way, and interpret them by the nearest analogy in our own experience, is the error of 'anthropomorphism.' It was common amongst the earlier investigators of the minds of animals; and the danger of it (I believe) G. H. Lewes first clearly exposed in *Problems of Life and Mind* (Third Series: *The Study of Psychology*, c. viii.). But freedom from anthropomorphism can never be more than a matter of degree. We must either give up the attempt to understand the subjective side of an animal's behaviour; or else, with whatever precautions, find some analogy in our own consciousness. Some 'analogy,' however, means some functional equivalence, not necessarily the same sense-modality, quality of emotion, etc. Does a grasshopper 'hear,' for example? It does hear; but it may not hear tones as we do.

Other discouragements of our study, besides those that beset most empirical sciences, arise from the expense of time and trouble that must often be put up with in obtaining the simplest fact. If we work by experiment in the laboratory, the animals sometimes (as one of my colleagues observed) seem to take no interest in the proceedings. If we betake ourselves to observation in the field, hour after hour may pass with no apparent result, except the roasting of our own necks in the sunshine, of which even such enthusiasts as Fabre and the Peckhams complain. All naturalists are agreed upon the need of endurance: it is

generally called 'patience'—a most misleading term. Patience is a passive quality, characteristic of those who put up with evils which they regard as irremediable, or which they have no courage to oppose: the weather, or the government. But the watching and waiting of a naturalist is the perseveration of an instinct, like the anchorage of a cat at a mouse's hole: it is the eager desire to know, overcoming all allurements, discouragements and distractions.

If these drawbacks to the study of Animal and Comparative Psychology deter anyone from pursuing it, we may parody a saying of Kant's: "it is not necessary that every man should be an Animal Psychologist"; but whoever neglects it will never get to the bottom of human nature. Not that to understand mankind is the sole motive to study animals: they well deserve study for their own sake: but it seems to me to add (at least) to the value of such investigation that, through the Comparative Method, it will enable us to know ourselves.

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SOME OBSERVATIONS ON LOCAL FATIGUE IN ILLUSIONS OF REVERSIBLE PERSPECTIVE.

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Local fatigue as manifested in the 'Windmill' or 'Revolving Cross' Illusion. McDougall's observations confirmed. Local fatigue not manifested by all subjects. Differences of local fatigability in the same subject. Experiments showing the extremely specific nature of this local fatigue. Local fatigue unaffected by other simultaneous mental processes. The rôle of local fatigue in illusions of reversible perspective. The general interest of local fatigue. Summary.

IN a paper in the last number of this *Journal*¹ a considerable amount of experimental evidence was brought forward, indicating that the direction of the attention is a factor of great importance in illusions of reversible perspective. In the course of a long investigation with several trained observers, a very close correspondence was shown to exist between attention to any part of a reversible figure and the forward appearance of that part; while reversals of perspective were found to coincide with observable movements of the attention from one part of the figure to another. It was also pointed out that these results gave rise to a number of interesting problems, which had not been dealt with in that paper.

Among the problems immediately concerned with the direction of the attention itself, was the question:—Why is it that the attention cannot be steadily maintained upon any one part of the figure, so as to maintain the same perspective indefinitely, if the observer so desires? On turning to a consideration of this question at the conclusion of the

¹ J. C. Flügel, "The Influence of Attention in Illusions of Reversible Perspective." *This Journal*, 1913, v. 357.

previous research, it seemed to the present writer that the answer was very possibly to be found in the phenomenon of local fatigue brought to light by the researches of McDougall¹. According to this author, the inability to prevent reversals of perspective is due to the rapid rise of fatigue in the higher nervous paths subserving the perspectives, which produces in turn a series of rapid changes in the relative resistances of the two systems; the whole process being only a particular case of that general instability which is found throughout the higher nervous arcs.

This explanation would seem to be readily applicable to our present problem, the only change required being that the fatigued systems should correspond not only to the perception of the perspective but also to the clearness of the sensory presentation of a particular part of the figure (since it is here not only the change of perspective but also the accompanying movement of attention from one part of the figure to another, which has to be explained). This change seemed the easier, in so far as McDougall had already demonstrated the existence of local fatigue, even on the purely sensory level, in some of his observations on binocular rivalry². In view of these considerations, it was decided to repeat McDougall's observations on local fatigue in illusions of reversible perspective with some of the subjects of our own previous experiments. It was hoped that, by so doing, we might obtain direct evidence as to the influence of local fatigue upon the direction of the attention, and that we might at the same time be able to throw some light upon various other problems arising out of our previous results, such as the cause of the marked individual differences in the power of controlling the attention and the relation of involuntary changes in the direction of the attention to the "fluctuations of attention" in general. The present paper is devoted to a brief description of the experiments thus initiated³.

McDougall's demonstration of local fatigue rests upon the following considerations. If the reversals of perspective are due to the rapid rise

¹ "The Physiological Factors of the Attention Process." *Mind*, 1906, xv. 329. Especially 340 ff.

² *Op. cit.* 341.

³ I take this opportunity of thanking those who, undeterred by the fact that these were experiments in 'fatigue' (although, it is true, of a rather less trying nature than is usual in such cases), were kind enough to offer themselves as observers. Where not otherwise stated, these were all trained psychologists or advanced students of psychology. My thanks are also due to Professor Spearman for much valuable help and advice throughout the investigation.

of resistance in the nervous paths corresponding to the alternative perspectives, it would seem that, if by any means one path can be kept in use for a longer period than usual, then when the reversal of perspective does at length occur, a correspondingly long period would be required for the unusual resistance to become reduced to the normal level, *i.e.* for the effects of the fatigue to wear off. In other words we should expect an unusually long period of one perspective to be followed by an unusually long period of the other. In the drawings and diagrams most frequently used for the study of these illusions, it is difficult or impossible to produce an exceptionally long period of one perspective. This can be done however with comparative ease in the case of those illusions of reversible perspective which occur with objects in three-dimensional space. Under these circumstances it is found (as was shown also by our own observations with the model of the prism figure recorded in the previous paper, p. 394) that the 'illusory' perspective occurs much more frequently when the object is regarded with one eye than when it is regarded with two. In the latter case the 'normal' perspective can frequently be held for long periods without reversal. If after such a long period of 'normal' perspective with binocular regard, we close one eye and thus produce a state of more equal opportunity as regards the two perspectives, we should expect in turn an unusually long period of 'illusory' perspective; and this is in fact what occurred in McDougall's observations.

For the purpose of these experiments McDougall made use of the 'windmill' illusion or revolving cross. If a windmill or revolving cross be regarded unocularly and obliquely, it can be observed that the movement of the sails or arms of the cross apparently changes its direction from time to time, the movement of the sails in the upper part of their orbit being sometimes towards the observer, sometimes away from him. This is, in reality, an illusion of reversible perspective, the change in the direction of the movement being coincident with a change in the apparent spatial relations of the windmill, that side of it which before reversal appeared the nearer to the observer appearing, after reversal, to be the further from him, and *vice versa*¹.

In our own experiments a cardboard cross was used, the four arms of which were each 30 cm. long and 2 cm. broad, and which was rotated by an electric motor at the rate of about one revolution in five

¹ The illusion can of course be observed when the windmill or cross is stationary, but the reversals are in this case less marked, probably because the changes of perspective are no longer reinforced by apparent changes in the direction of the movement.

seconds. The observer sat at a distance of $2\frac{1}{2}$ m. and at an angle of 30° from the centre of the cross. The changes of perspective were recorded on a smoked drum in the manner described in the previous paper (p. 360). Care was taken that the illumination of the cross and of the whole room should remain uniform in all the experiments. Each complete experiment consisted of three parts:—(1) the 'preliminary' or 'normal period,' made unilocularly and in an unfatigued state; (2) the 'fatigue-inducing period,' during which the subject regarded the cross binocularly; and (3) the 'test period,' in which the conditions were similar in all respects to those of the preliminary period, except that the psycho-physical system corresponding to the 'normal' perspective was now (we may suppose) in a fatigued state owing to its having been in exercise during the whole or the greater part of the preceding fatigue-inducing period. The preliminary period occupied one minute, the fatigue-inducing period five minutes; the test period, like the preliminary period, occupied one minute, except when it appeared specially desirable to extend it over a longer time. In some of the earlier experiments (those recorded in Table I) an interval of ten seconds was allowed to elapse between the end of the fatigue-inducing and the beginning of the test period, during which the subject looked away from the cross; but in the majority of the experiments the test period followed immediately upon the fatigue-inducing period. The manifestations of fatigue do not however seem to have been appreciably affected in any way by the presence or absence of this interval. A number of preliminary experiments had shown that the direction of the attention was effective in controlling the perspective in the 'wind-mill' illusion in the same way and to about the same extent as with the models of the prism figure employed in the previous research. It therefore became important that the conditions of attention should be as far as possible the same in the preliminary and in the test periods. To facilitate this a fixation mark was provided in both cases, and the subject was told to attend principally to the immediate neighbourhood of this fixation point. The position of the fixation mark was not always the same with different subjects, as it was desirable for the present purpose to allow the fixation (and consequently the attention) to be such as to obtain a fair proportion of both perspectives during the preliminary period, and this was found to necessitate a different position with different subjects. The position of the fixation mark, when once determined, remained however the same during the preliminary and the test periods, and the subjects were instructed to make, as far as possible, the same effort to fixate and attend in both cases.

64 *Fatigue in Illusions of Reversible Perspective*

The results of the first experiments conducted in this way are shown in Table I. In this table, as in those of the previous paper, the figures represent the proportion of the total period of observation during which the cross was seen in either perspective. For ease of comparison these proportions are in each case expressed as percentages of the total period of observation. The two perspectives are here described as 'right forward' and 'left forward' respectively. By 'right forward' is meant that perspective in which the right hand end of the cross is seen nearer the observer, by 'left forward' that in which the left hand end is seen nearer. In this case the 'right forward' was the 'normal' or 'real,' the 'left forward' being therefore the 'illusory' perspective.

TABLE I.

Subject	C. S.		N. C.		A. W.		W. G.		C. R.		J. C. F.		F. A.		C. L. B.		M. W.	
Perspective	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R
Preliminary period	44	56	61	39	48	52	37	63	40	60	57	43	55	45	59	41	37	63
Test period (after fatigue of R)	81	19	58	42	62	38	37	63	62	38	58	42	47	53	87	13	41	59

Figures represent percentage of total period of observation during which either perspective was seen. L='left forward,' R='right forward.' In this case 'right forward' was the fatigued perspective, so that fatigue, when present, should manifest itself in an increase of L and decrease of R during the test period as compared with the preliminary period.

Of nine subjects four showed fatigue much in the same way as McDougall's observers. The other five however showed no signs of any such fatigue, the relative amounts of the two perspectives in the test period being much the same as in the preliminary period. In view of these apparent individual differences it seemed desirable to repeat the experiments with a larger number of subjects. As it was not found possible to obtain further trained subjects for this purpose and as in these observations, which in this respect were unlike most of the experiments recorded in the previous paper, delicate introspection was not essential, arrangements were made with a number of boys of from 12½ to 14 years of age, who came to the Laboratory from a neighbouring school¹. After a little practice these boys appeared to become

¹ I am greatly indebted to Mr E. Rendell, Headmaster of Stanhope Street School, and to Mr W. Welborne, Master at the same school, for their kindness in selecting suitable subjects from among their pupils, and for making the necessary arrangements with them as regards visiting the Laboratory.

quite capable of making and recording their observations with sufficient accuracy, and with scarcely an exception approached the task with considerable interest and goodwill¹. They were however not informed of the purpose of the experiments nor in any way led to conjecture as to the nature of the results to be expected.

At the same time the experimental procedure was elaborated by the addition of a second fatigue-inducing period, during which the 'left forward' perspective was fatigued, the effect of this fatigue being measured in a second test period, just as the fatigue of the 'right forward' perspective had been measured before. This was done with the help of a second cross of similar construction to the first, driven by a separate motor and placed in such a position that, when viewed by the subject, it presented precisely the same appearance when in its 'normal' perspective as did the original cross in its 'illusory' perspective. For the sake of uniformity, the second cross was also used when fatigue was induced for the 'right forward' perspective. The relative position of the two crosses in both cases will be made clear by the accompanying simple diagram, where the arrangements correspond to those represented in *a* and *b*². Thus in *a* the cross used in the fatigue-inducing period ('fatigue-inducing cross') is so placed as to appear similar to the cross used in the test period ('test cross'), when the latter was seen 'normally.' On turning from the former to the latter cross at the end of the fatigue-inducing period, the condition of the

¹ In estimating the reliability of the results obtained with these subjects, we need not however trust only to the general impression of the experimenter. An objective guarantee of the reliability seems to be afforded by the two following circumstances:—(1) the experiments were performed at least twice with each subject at an interval of over a week and the results obtained from the two sittings are (as will be seen from Table II), with only two exceptions, remarkably consistent with each other; (2) the same individual differences and peculiarities as are exhibited by the boys are shown also by the adult trained observers.

² In this diagram it is of course in every case the *real* position of the crosses which is indicated. By the aid of the diagram it is therefore easy to apprehend the appearance of the crosses when in their normal perspective; to realise their appearance during the illusory perspective it should be borne in mind that both the perspective itself (*i.e.* right or left side forward) and the direction of the movement appear reversed. It was however only the test cross which could be seen in this illusory perspective, since this cross alone was regarded unocularly; the fatigue-inducing cross, being always viewed binocularly, was (for all practical purposes) seen only in its normal perspective. As regards the manifestations of fatigue, it is evident that where the normal perspective of the two crosses is the same (as in *a* and *d*), fatigue must show itself by an increase of illusory perspective of the test cross; on the other hand, when the normal perspective of the two crosses is different, *i.e.* when one is 'right forward' and the other is 'left forward' (as in *b* and *c*), fatigue must be manifested by an increase of the normal perspective of the test cross.

observer is such that the system corresponding to the 'normal' perspective of the test cross has been in use for some time, while the system corresponding to the 'illusory' perspective is unfatigued. In *b* the fatigue-inducing cross is placed so as to appear similar to the test cross when the latter is seen in its 'illusory' perspective: it is now therefore the 'illusory' system which has been used and the 'normal' which is unfatigued at the beginning of the test period. The complete series of observations, including one preliminary, two fatigue-inducing,

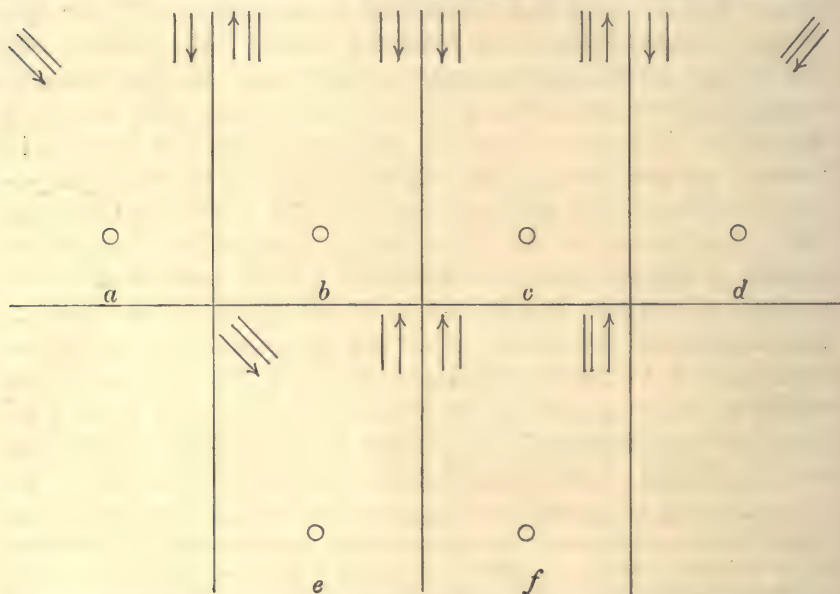


Diagram to show the various arrangements of the crosses used in the experiments.

| = 'Preliminary' and 'Test' Cross.

|| = 'Fatigue-inducing' Cross.

↑ = Direction of movement of upper part of Cross.

○ = Observer.

and two test periods, was made twice with each subject. At each sitting there was an interval of not less than 35 minutes between the end of the first test period and the beginning of the second fatigue-inducing period. At the first sitting the 'right forward' perspective was fatigued first, at the second sitting the 'left forward.'

The results of these experiments are recorded in Table II. This Table shows that the results obtained from subjects 1 and 2 were irregular, but that the other subjects exhibit quite a high degree of consistency between the results of the two sittings¹. After the completion

¹ This consistency holds not only for the percentage of the two perspectives but also for the average duration of the individual periods (not shown in the table).

of the whole series of experiments, the first two subjects were each given two more sittings, both of which produced results consistent with themselves and with the second of the two previous sittings, so that we may conclude that it was only in the first sitting that these subjects showed insufficient training for our purpose.

TABLE II.

Subject	1		2		3		4		5		6		7		8	
	L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R
Preliminary	62	38	36	64	69	31	33	67	38	62	13	87	43	57	63	37
Test (after fatigue of R)	100	0	55	45	67	33	64	36	65	35	68	32	42	58	48	52
Test (after fatigue of L)	100	0	86	14	43	57	10	90	22	78	0	100	34	66	43	57
Preliminary	70	30	38	62	51	49	30	70	55	45	32	68	47	53	68	32
Test (after fatigue of R)	67	33	38	62	42	58	59	41	100	0	55	45	43	57	49	51
Test (after fatigue of L)	67	33	12	88	30	70	22	78	15	85	15	85	41	59	54	46

Subject	9		10		11		12		13		14		15	
	L	R	L	R	L	R	L	R	L	R	L	R	L	R
Preliminary	50	50	38	62	45	55	46	54	36	64	56	44	48	52
Test (after fatigue of R)	43	57	52	48	46	54	49	51	72	28	56	44	52	48
Test (after fatigue of L)	47	53	43	57	48	52	35	65	41	59	54	46	58	42
Preliminary	55	45	53	47	42	58	44	56	43	57	48	52	50	50
Test (after fatigue of R)	42	58	54	46	45	55	46	54	62	38	48	52	45	55
Test (after fatigue of L)	42	58	54	46	60	40	27	73	46	54	49	51	51	49

Figures and indications as before. Fatigue of R corresponds to *a* in the diagram, fatigue of L to *b*.

As a result of these experiments, the fifteen subjects seem to fall into three fairly distinct classes. The first and largest class, which includes eight subjects (Nos. 1, 7, 8, 9, 10, 11, 14 and 15), comprises those who exhibit no appreciable or only very slight signs of fatigue. Into the second class fall three subjects (Nos. 4, 5 and 6) who show

distinct signs of fatigue in the case both of *a* and of *b*. The remaining subjects (Nos. 2, 3, 12 and 13), who constitute the third class, show the usual signs of fatigue with one arrangement, while manifesting no fatigue or distinctly less fatigue with the other arrangement. Of these subjects No. 13 shows the greater fatigue with arrangement *a*, and the other three with arrangement *b*. Taking the two last classes together, the proportion of fatigued to unfatigued subjects is fairly comparable to that found with the adult trained observers, and the results from both sets of experiments agree in showing that the phenomenon of local fatigue, as brought out in these and in McDougall's experiments, is by no means manifested by all persons, those who do not manifest fatigue being, at least in the present case, in the majority.

It is of course possible that if the fatigue-inducing period had been still further prolonged, fatigue would eventually have been manifested even by the latter. But even if this were the case, it is evident that these subjects possessed an amount of resistance to local fatigue, which would make it quite impossible to account for the ordinary fluctuations of perspective, as observed by these subjects, in terms of such fatigue: it is clear in fact that, with these subjects, local fatigue can play no appreciable part in the reversals of perspective under ordinary conditions.

It would seem therefore that we cannot assign to local fatigue any very important part in the fluctuations of perspective, as these are generally observed, since it is probable that in many persons it is not generally present to any appreciable degree, and comes into play, if at all, only on occasions when, for any reason, one perspective has been retained to the exclusion of the other, for a period that is very much longer than usual.

Although local fatigue seems thus to be a factor of minor importance only in illusions of reversible perspective, the phenomenon is, as will be seen, of considerable interest on its own account and in connexion with the whole theory of fatigue.

Perhaps the most curious fact indicated by the experiments upon the boys, which we have just described, is the apparent co-existence in the same individual of marked fatigability to one perspective with very small or inappreciable fatigability to the other. On repeating the experiments upon fatigability to both perspectives with some of the trained observers, upon whom we had experimented in the first place, and with one other who had since become available, we discovered two cases in which one perspective was more fatiguing than the other, and one case of fatigability to one perspective combined with apparent

phenomenon
of local
fatigue

complete non-fatigability to the other. As in the case of the boys, the individuals differed as to which perspective was found the more fatiguing. The fatiguing perspective however remained constant for the same individual, even when the experiments were repeated after a very considerable interval of time (over two months in the case of two of the adult subjects), showing that the greater fatigability to one perspective was not due to any merely temporary cause but was a permanent characteristic of the individual.

In view of this corroboration of the interesting phenomenon of one-sided fatigue, there arises the question:—Is it possible to determine more precisely the exact conditions of this difference of fatigability? Perhaps the most striking difference between the arrangements shown as *a* and *b* in the diagram is that in the one case (*a* in diagram) fatigue manifests itself in an increase of the 'illusory' perspective of the test cross, in the other (*b* in diagram) in an increase of the 'normal' perspective. It might be supposed at first sight that the apparent differences of fatigability are really due to differences in the ease with which fatigue can be manifested, according as it results in an increase of 'normal' or an increase of 'illusory' perspective. The fact that some observers are more fatigable with arrangement *a*, others with arrangement *b*, shows, of course, that there is no universal tendency for an increase of 'normal' to be produced more easily than an increase of 'illusion' or *vice versa*. It remains possible however that such a tendency may exist in any particular subject, since we have not yet shown that fatigue may result sometimes in an increase of 'normal,' sometimes in an increase of 'illusion' *in the same observer*.

The proof of this however is afforded by some experiments with the two new arrangements depicted in *c* and *d* (diagram, p. 66), where the test and fatigue-inducing crosses have been transposed, the test cross being now to the left of the observer and the fatigue-inducing cross to the right. As a result of these new observations it was found that the two subjects who displayed greater fatigue in *b* than in *a* also displayed greater fatigue in *d* than in *c*, though fatigue resulted, in the case of *b*, in an increase of 'normal,' in the case of *d*, in an increase of 'illusion.' The remaining subject, who had shown distinct fatigue in *a* combined with absent or inappreciable fatigue in *b*, showed, correspondingly, distinct fatigue in *c* and little or no fatigue in *d*: in this case the fatigue manifested itself in *a* by an increase of 'illusion,' in *c* by an increase of 'normal.' The numerical results obtained from the three subjects with all four arrangements (*a*, *b*, *c* and *d*) are shown in Table III.

TABLE III.

	Arrange- ment	Test Cross, right		Arrange- ment	Test Cross, left	
Subject: J. K.		L	R		L	R
Preliminary		56	44		39	61
Test (after fatigue of R)	<i>a</i>	80	20	<i>c</i>	100 (78)	0
Test (after fatigue of L)	<i>b</i>	0	100 (162)	<i>d</i>	0	100 (240)
Subject: A. W.						
Preliminary		31	69		45	55
Test (after fatigue of R)	<i>a</i>	67	33	<i>c</i>	62	38
Test (after fatigue of L)	<i>b</i>	0	100 (120)	<i>d</i>	0	100 (97)
Subject: C. S.						
Preliminary		47	53		47	53
Test (after fatigue of R)	<i>a</i>	85	15	<i>c</i>	100 (131)	0
Test (after fatigue of L)	<i>b</i>	45	55	<i>d</i>	50	50
(2nd series)						
Preliminary		50	50		48	52
Test (after fatigue of R)	<i>a</i>	71	29	<i>c</i>	89	11
Test (after fatigue of L)	<i>b</i>	53	47	<i>d</i>	37	63

Main figures as before. The figures in brackets represent the actual duration in seconds of the appearance of any particular perspective, when this perspective was visible during the whole period of observation. In all such cases this period was prolonged until a change of perspective did at length occur. The letters (*a*, *b*, *c*, *d*) refer to the arrangement of the crosses with reference to the observer (see diagram p. 66).

These results seem to afford fairly conclusive evidence that the phenomenon we are considering is not due to any difference in the ease with which fatigue can manifest itself according as it results in an increase of 'normal' or of 'illusory' perspective during the test period. We may therefore assume that our results are due to genuine differences of fatigability.

Turning now to the actual perceptive elements concerned in the reversals, it would seem that they can be split up into two independent factors:—(1) the perspective proper (i.e. 'right' or 'left forward') [that this is an independent factor in the reversals is shown by the fact already mentioned, that reversals can take place in the absence of any movement of the cross]; (2) the direction of the movement (e.g. the top of the cross can be moving towards or away from the observer).

Let us consider first the part played by the perspective in the manifestations of fatigue. A study of Table III and of the diagram will show that the relations of fatigue to this factor of the perspective

remain constant in each of the three subjects. With C. S. fatigue is manifested on every occasion when 'right forward' is seen during the fatigue-inducing period (*a* and *c*), while fatigue does not appear when 'left forward' is visible during this period (*b* and *d*). With J. K. and A. W., on the other hand, greater fatigue is shown when 'left forward' is seen during the fatigue-inducing period (*b* and *d*), less fatigue when the 'right forward' is seen (*a* and *c*). The presence of this constant relation between the fatigue and the perspective seems to indicate that this factor of the perspective is, as we might expect, an essential element of the fatigable system.

As regards the second of the two above-mentioned factors, it will be seen that there is no such constant relation to the fatigue as in the case of the perspective. Thus in *a* the movement of the fatigue-inducing cross is towards the observer, in *c* it is away from the observer, though C. S. manifests fatigue equally in both cases. The movement similarly differs in the case of *b* and *d*, though these two arrangements correspond in giving the greater fatigue with observers J. K. and A. W. If the direction of the movement were the essential factor, we should expect *a* to correspond with *d* and *b* with *c*, whereas with all three observers the correspondences actually found are between *a* and *c* and *b* and *d*, where (as we have already seen) the perspective and not the movement is the common factor. Our observations seem then to indicate conclusively that (in the present cases at least) it is the perspective of the cross, and not the direction of the movement, which is principally concerned in these differences of fatigability.

There remains the question whether the direction of the movement gives rise to any fatigue at all, similar in nature to that brought about by the perspective. The results recorded in Table III, as we have just seen, fail to show any manifestations of such fatigue. It would appear possible however to put the matter to a further test. If the direction of the movement plays no part whatever in the production of fatigue, we should expect that the manifestations of fatigue brought about by the perspective would be independent of whether they resulted in an increase of movement in one direction or in the other during the test period.

For the purpose of applying this test the crosses were arranged as in *e* and *f*, which are similar to *a* and *c* respectively, except that the upper part of the test cross is moving away from, instead of towards, the observer. In *a* fatigue was produced for the 'right forward' perspective and manifested itself in an increase of 'left forward,'

72 *Fatigue in Illusions of Reversible Perspective*

together with movement away from the observer, both perspective and movement being reversed. If now the perspective is the only element concerned in the fatigue, we should expect a similar increase of 'left forward' in *e*. In the case of *e* however the reversal would affect the perspective only and not the movement, since 'left forward' (which in *e* is the 'illusory' perspective and therefore involves an apparent movement in the opposite direction to that in which the cross is really moving) must be accompanied by movement of the top towards the observer, *i.e.* in the same direction as the movement seen during the fatigue-inducing period. If, on the other hand, reversal of movement, as well as of perspective, is in any way important for the manifestation of fatigue, we should expect that no fatigue would be shown in *e*, or at any rate that it would be exhibited to a less marked extent than where (as in *a*) both perspective and movement can be reversed simultaneously. Precisely similar considerations apply to *f*, except that in this case fatigue of the perspective would manifest itself (as in *c*) by an increase of the 'normal' instead of the 'illusory' perspective in the test period.

Some results obtained with subject C. S. with arrangements *e* and *f*, together with (for the sake of comparison) a further record with arrangement *a*, taken at the same time, are given below:—

	<i>a</i>		<i>e</i>		<i>f</i>	
	L	R	L	R	L	R
Preliminary	45	55	50	50	47	53
Test	94	6	46	54	49	51

It will be seen that no fatigue is manifested with *e* or *f*, though there is, as before, well marked fatigue in the case of *a*. These results seem to indicate fairly clearly that the different constituents of the total perception corresponding to the perspective and the movement respectively are both independently but simultaneously fatigued, and that the effects of these two separately fatigued elements neutralise one another in the test record. Thus in *f* fatigue to the perspective would tend to make the test cross appear in the 'normal' phase; fatigue to the movement, on the other hand, would simultaneously tend to produce the 'illusory' phase.

It may appear perhaps at first sight that this interpretation is contradicted by the results obtained from the same observer with arrangements *c* and *d*, which, as we saw, failed to exhibit any manifestations of fatigue due to the direction of the movement, though in these cases there was apparently nothing to prevent the manifestation of such fatigue, had it existed. It must be admitted that the results

obtained with arrangements *e* and *f* would have been more easily explained, if there had been some manifestations of fatigue with *b* and *d* also. The absence of such manifestations in the latter case is not, however, incompatible with the existence of fatigue due to the movement, such as seems indicated by the results with *e* and *f*, if we assume (as would seem probable in any case) that there exists a definite and more or less constant threshold, which must be passed, before fatigue can manifest itself by an increase of one or other of the two perspectives. We may then suppose that in the present observer the fatigue caused by the direction of the movement is subliminal, and therefore fails to manifest itself in *b* and *d*; that the fatigue caused by the perspective 'right forward,' which, as we saw, is with this subject greater than that caused by 'left forward,' is supraliminal, either by itself or when combined with that due to the movement, and therefore manifests itself in *a* and *c*; but that this fatigue of 'right forward' is no longer supraliminal when, as in *e* and *f*, its effects are no longer strengthened but opposed by the fatigue due to movement. This will be readily understood, if expressed in actual figures. Let us suppose that the threshold is passed, and fatigue therefore manifested, as soon as the fatigue effects due both to inclination and to movement reach a total of 30. Let us suppose further that the fatigue of 'right forward' produced in five minutes amounts on any given occasion to 20, that of 'left forward' to 10, and that of the movements towards and away from the observer (between which there is, in the present case, no reason to assume the existence of any considerable difference) in both cases to 15. Then in the case of *a* and *c* the total fatigue will be $20 + 15 = 35$, being above the threshold; in the case of *b* and *d* it will be $10 + 15 = 25$, which is below the threshold. In *e* and *f* the effects of the fatigue due to perspective and the movement respectively are working in *opposite* directions; therefore, instead of $20 + 15$, as in *a* and *c*, we get $20 - 15 = 5$, which is, of course, below the threshold. The equation thus works out correctly in every case, affording very considerable confirmation of the view here advanced. Our data of course afford us no indications of the actual relative fatigability of the different systems, except such as can be deduced from the facts that (in this case) fatigue to 'right forward' *plus* fatigue to movement is above the threshold, while fatigue to 'right forward' *minus* fatigue to movement and fatigue to 'left forward' *plus* fatigue to movement are both subliminal; there are, of course, a very large number of possible relations between the systems which will meet these conditions.

Naturally I am aware that this view may have to be greatly modified as the result of further observations: I have advanced these considerations chiefly with the object of showing that the results obtained with *b* and *d* and with *e* and *f* are not necessarily mutually contradictory, as they might perhaps at first sight appear.

Summing up the results from this series of observations, we may then conclude:—

(1) That the phenomenon of one-sided fatigue is not due to any difference in the ease of manifestation, according as it results in an increase of 'normal' or an increase of 'illusory' perspective.

(2) That the greater fatigability to one perspective is (at least in our present three observers) chiefly connected with the perception of the perspective of the cross (*i.e.* 'right' or 'left forward').

(3) That the perception of the direction of the movement is nevertheless, in all probability, independently fatigable.

One further experiment may be briefly referred to before concluding this paper. Since our data all tend to indicate that the fatigue we are here considering is extremely local and specific in nature, it would be interesting to know how far this local fatigue is affected by other simultaneous mental processes. We therefore determined to see whether the amount of attention given to the cross during the fatigue-inducing period had any influence on the amount of the fatigue. For this purpose one or two distraction experiments were made, in which the subject (in this case A. W.), while fixating the cross in the usual way, was asked to give his attention to some other matter during the fatigue-inducing period. Two kinds of distraction were tried:— (1) attentive listening to a story read aloud by the experimenter, (2) adding 3's aloud as quickly as possible. The first produced a mild, the second a very high degree of inattention to the cross. The results obtained in this way were as follows¹:—

	Listening to story		Counting 3's		No distraction	
	L	R	L	R	L	R
Preliminary	65	35	66	34	58	42
Test (after fatigue of R)	90	10	77	23	85	15
Test (after fatigue of L)	13	87	0	100 (72)	0	100 (69)

It is fairly evident that the fatigue is unaffected by the distraction.

¹ It should be noted that these results agree with those for the same subject in Table III in showing greater fatigue for 'left forward' than for 'right forward,' though these later observations were made after an interval of over a month.

We must conclude therefore that the local fatigue, with which we are here dealing, is independent of the general direction of the cerebral energy, and runs its course whenever the particular perceptive element is called into play, without reference to the way in which the rest of the conscious energy is occupied¹.

Perhaps the chief point of interest that emerges from all our observations on local fatigue, as manifested in the 'revolving cross' illusion, is the extremely specific nature of this fatigue. This specificity is clearly indicated by the following considerations:—(1) that fatigue is manifested in some individuals and not in others: (2) that in the same individual one perceptive system may be highly fatigable, while another very similar system may be much less fatigable, or even show an altogether inappreciable degree of fatigue: (3) that individuals differ as to which of these two perceptive systems is the more fatigable: (4) that this fatigue is independent of the simultaneous activity of other centres. It would seem that this extreme specificity of certain kinds of fatigue cannot but be of very considerable physiological and psychological interest. The fact that one perceptive system (*e.g.* 'right forward') can differ considerably from another apparently very similar perceptive system (*e.g.* 'left forward') in such an important matter as fatigability seems to indicate that there must exist important differences of structure or function between neighbouring and closely connected nervous paths. This again seems to point to the probably very specific nature of certain functions and abilities—a conclusion which, we may note, is in harmony with the results obtained from much work that has been done on 'mental tests' in recent years, and which is also of importance in view of the problems connected with the parts played by 'general' and 'specific' factors in these tests and in mental performances generally².

As regards the question with which we are ourselves more immediately concerned, *i.e.* the relation of this local fatigue to the

¹ It is interesting to compare this result with that obtained from similar observations on the after effect of movement in a plane at right angles to the line of vision. (See A. Wohlgenuth, "On the After Effect of Seen Movement." *This Journal, Monograph Supplements*, No. 1, p. 83.) In neither case has distraction any influence on the after effect, though the three-dimensional space perception involved in the present experiments is a 'higher' process than that concerned in Dr Wohlgenuth's observations, in so far as it is acquired by experience and not innately organised, as is probably the case with the perception of movement in two-dimensional space.

² See, for instance, Bernard Hart and C. Spearman, "General Ability, its Existence and Nature." *This Journal*, 1912, v. 51.

principal factor in the reversibility of perspective, namely, the direction of the attention, I am inclined to believe that the two factors work as a rule in almost complete independence of one another, and that, at any rate, they have little or nothing in common in their nature. As already shown, the manifestations of local fatigue vary too much both as regards the differences between individuals and the differences between different perceptive systems in the same individual to allow us to suppose that they underlie the involuntary changes in the direction of the attention observed in every one under normal circumstances. There is reason to believe moreover that the factors concerned in the voluntary maintenance of attention are very general; while local fatigue is, as we have seen, extremely specific. Nor is there any indication that there exists any correlation between individual peculiarities as regards the one and as regards the other. We have shown in the earlier paper that there exist considerable individual variations in the power of controlling the movements of attention, which result in changes of perspective. Those who show local fatigue are to be found both among those who exhibit high (*e.g.* A. W.) and low (*e.g.* C. S. and C. R.) power of controlling the attention. Similarly, as regards those who do not show local fatigue (*e.g.* N. C. and J. C. F. respectively). In general, so far as the data at present available enable us to judge, everything seems to show that there is very little connexion between the two factors, which are also, apparently, of very different importance as regards illusions of reversible perspective. The one is, it would seem, continuously operative in all cases of reversible perspective; while the other, in all probability, only comes to exert an appreciable influence with certain persons, and under certain special conditions (such as that of our long 'fatigue-inducing period') not present in the majority of observations on these illusions.

Although the phenomenon of local fatigue seems thus to play only a minor part in illusions of reversible perspective, the facts concerning this local fatigue brought out by the present observations are, I venture to think, not without very considerable general interest. Besides the facts indicating the extreme specificity of the fatigue, to which allusion has already been made, there arise a number of further problems connected with the subject of local fatigue, one or two of which may be mentioned in conclusion. Thus it is evident that interesting problems are afforded by the questions as to the frequency with which other cases of this local fatigue are to be met, the levels of consciousness

at which they are most prevalent¹, the importance and complexity of the functions they affect and their relations to the wider phenomenon of 'general fatigue.' These problems too will, in all likelihood, afford no insuperable obstacles to experimental investigation, and, in so far as they are solved, can scarcely fail to throw very considerable light upon the whole nature of fatigue.

Summary.

Experiments on the 'windmill' or 'revolving cross' illusion confirm McDougall's observation that a prolonged period of binocular regard may produce fatigue of the aspect seen during this period, the fatigue being manifested in an unusually long period of the opposite aspect as soon as uniocular is substituted for binocular regard.

This fatigue however is not manifested by all the subjects of the present experiments.

Among those subjects who manifest fatigue, there are some who show greater fatigue with one aspect of the cross than with the other.

In these cases the differences of fatigability seemed to reside in the perception of the perspective, though there is evidence to show that the perception of the perspective and of the direction of the movement are both independently fatigable.

The more fatigable aspect varies from one individual to another, but remains constant for each individual.

The manifestations of fatigue are independent of whether they result in an increase of the 'normal' or of the 'illusory' perspective.

They are likewise independent of the amount of attention given to the cross, being unaffected by other simultaneous mental processes during the induction of fatigue.

These facts indicate that the local fatigue with which we are here concerned is highly specific in nature.

It is probably unconnected with the factors determining the direction of the attention, which (as was shown in a previous paper) is the main condition of the reversals of perspective under ordinary circumstances.

It is however of considerable interest on its own account.

¹ As already mentioned McDougall has shown that they are to be found on the purely sensory level.

BINOCULAR AND UNIOULAR DISCRIMINATION OF BRIGHTNESS¹.

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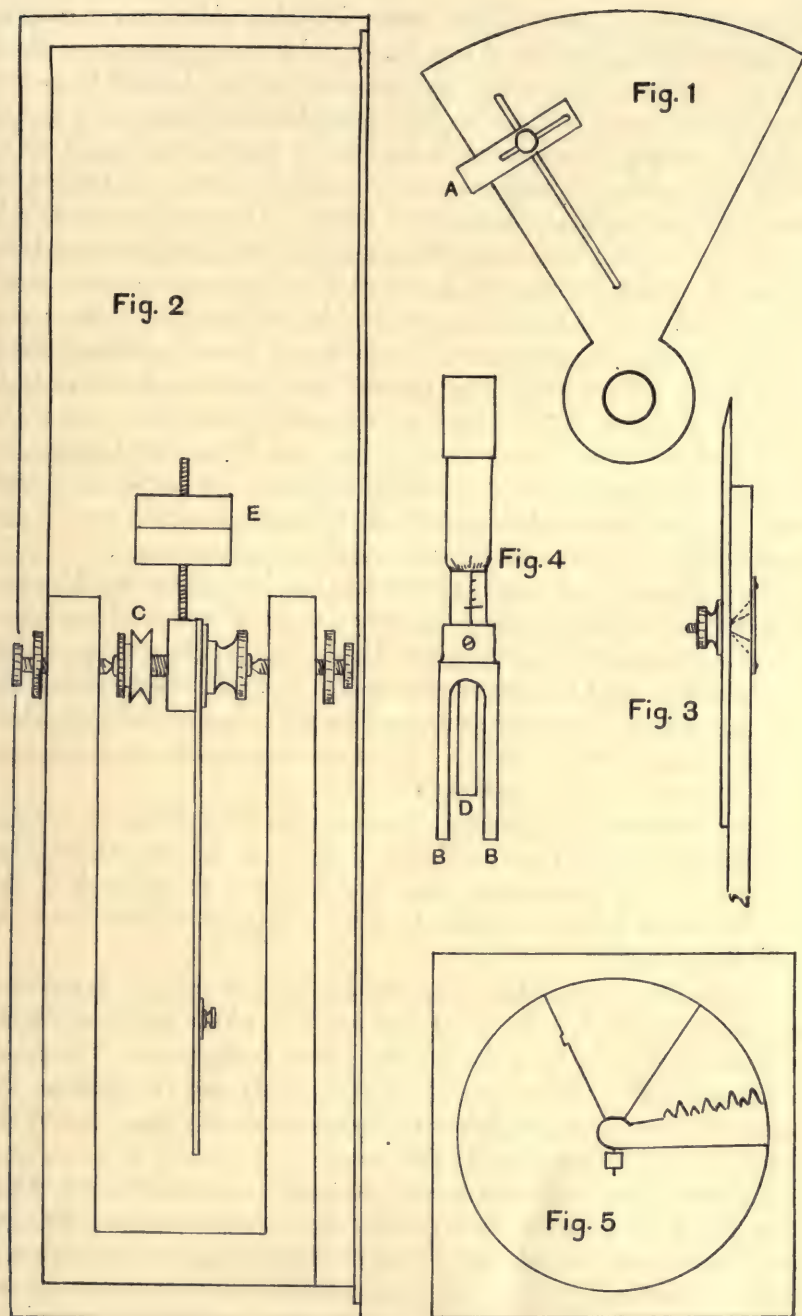
- I. *Description of apparatus.*
- II. *Method of conducting the experiments.*
- III. *Tabulated results, showing the differences between binocular and unioocular discrimination, as measured (i) by the frequency with which the grey to be discriminated was located with accuracy and certainty, and (ii) by the time taken in discriminating it.*
- IV. *Explanation of results; discussion of practice and the summation of the brightnesses of the unioocular images as possible explanations; an introspective basis for an explanation; suggested explanation; additional experimental evidence in support of it.*

THE following investigation has been made for the purpose of finding whether there are any differences between binocular and unioocular discrimination of shades of grey, and, if so, whether any experimental data can be found which will explain them. The experiments consist essentially in presenting to different subjects a grey ring of variable and measurable intensity on an otherwise uniform disc, and in asking them to discover its position and to describe as fully as possible its appearance.

I. DESCRIPTION OF APPARATUS.

The apparatus consists of an aluminium sector of angle 60° and radius 13 cms. rotating in front of a milk-glass screen behind which is a Kamm's incandescent lamp. Parallel to the edge of the sector, at a

¹ An abstract of this paper was read at Dundee before Section I of the *British Association for the Advancement of Science*, September, 1912.



FIGS. 1—5.

distance of 2.1 cms. from it, is a groove in which slides a screw passing through a rectangular strip *A* (see fig. 1) which can be placed anywhere along the edge of the sector and made to project beyond it to any extent up to 2 cms., and can be fixed in position by means of a screw-nut (see fig. 3). The groove is covered in front by a thin sheet of aluminium so that no light can pass through the sector. The adjusting screw is, therefore, at the back of the sector. The width of the strip is 1 cm. It is set in position and the amount of its projection beyond the edge of the sector is measured by means of the micrometer screw-gauge shown in fig. 4. The screw-gauge is used in the following manner: it is adjusted so that the distance of the surface *D* from the surfaces *BB* is equal to the amount which it is required that the strip should project; it is then placed so that the surfaces *BB* rest against the edge of the sector and the strip is pushed up to the face *D* and fixed there. In the first few experiments the projection was always re-measured after rotation of the sector, but after a little practice in the use of the apparatus had been gained this was found to be unnecessary.

The sector is fixed to an axle rotating on pivot points and is driven by a thin string passing round the groove *C* (fig. 2) and a driving wheel (not shown in fig.). The axle is held in position by two rigid horizontal bars, part of a solid framework of cast iron (fig. 5). On the side of the axle opposite to the sector is a screw on which are double nuts which act as a counterpoise (*E*, fig. 2). When the screws are carefully adjusted there is practically no vibration.

This framework is placed in a wooden box with a circular opening in front, so that only the disc of light over which the sector rotates can be seen. A milk-glass screen drops into a groove at the back of this box, where it is held in position by small clips, and whence it can be easily lifted out.

On the side of the projecting bar nearest the observer is gummed an indented strip of black paper the teeth of which project above the arm and stand out clearly against the bright background. The points of the teeth are at distances 5, 6, 7, 8, 9, 10, 11 and 12 cms. from the centre of rotation, and, to make them distinguishable from one another, they are made alternately high and low.

In front of the screen is a long, five-sided rectangular box which cuts off all light except that passing through the screen. The end nearest the observer is pierced by an elliptical hole just large enough to allow him to see the disc comfortably with both eyes: when only one eye is used this opening is covered by a piece of cardboard in which is a

circular hole 2.5 cms. in diameter. This screens off the observer's face from the rays passing through the disc, and so prevents reflection of light by the face; it has the further advantage of obviating the necessity of using an eye-cover which is disconcerting to many subjects. From a sheet of cardboard projecting over the head of the observer hangs a curtain of black cloth, which entirely excludes all light except that which passes through the screen. The whole of the apparatus—sector, iron framework and box—is painted dull black.

If the sector be rotated at a high speed when the strip *A* projects beyond its edge, there appears on the lighter background a grey ring the relative brightness of which can be varied by varying the amount of projection of the strip. By showing rings of different intensities and asking the observer to detect and locate them, the threshold for brightness discrimination can be found.

When we know the angle of the sector (60°), the distance of the projecting strip from the centre of the disc, and the amount it projects beyond the edge of the sector, the relative intensities of the ring and the background can easily be found. They have been calculated for the projections shown in Table I (the only projections used in the experiments), but only their 'difference-ratios' are here shown, *i.e.* the ratio of the difference of intensity of the ring and background to the intensity of the background (lower difference-ratio) or to that of the ring (upper difference-ratio).

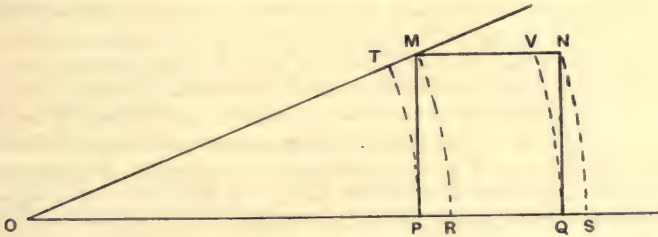


FIG. 6.

As one edge of the strip is parallel to that of the sector and the others are perpendicular to it, the grey band that is formed by rotating it is darker at its inner edge than at its outer, and these edges are not so sharp as they would be if the strip were an arc of a circle. In fig. 6 let *PMNQ* represent the projecting strip, and *OS* the edge of the projecting strip, and let *TP*, *MR*, *VQ*, and *NS* be arcs of circles with centre *O* at the centre of the disc. It is evident from the figure that the ring formed by rotating *PMNQ* about *O* will be darkest along the circle of

which MR is an arc, and will gradually increase in brightness to the part formed by rotating VQ . From P to R and from S to Q there is a very rapid fall in brightness. As, however, PR is never greater than 0.404 mm. and in these experiments did not subtend an angle of more than $86''$ at the eye of the observer, this is unappreciable. It is slightly greater than the maximum angle of discrimination, which Helmholtz found to be $65''\cdot75$. No blurring of the edges could be detected in the largest and darkest ring, in which, of course, PR has its greatest value.

TABLE I.

Radius	10	9	8	7	6	Lower ratios		Upper ratios
						<i>A</i>	<i>B</i>	<i>C</i>
Amount of Projection	·10	·09	·08	·07	·06	·0019	·0017	·0019
	·20	·18	·16	·14	·12	·0038	·0035	·0038
	·30	·27	·24	·21	·18	·0057	·0052	·0058
	·40	·36	·32	·28	·24	·0076	·0069	·0077
	·50	·45	·40	·35	·30	·0095	·0087	·0096
	·60	·54	·48	·42	·36	·0114	·0104	·0116
	·70	·63	·56	·49	·42	·0133	·0122	·0135
	·80	·72	·64	·56	·48	·0152	·0140	·0155
	·90	·81	·72	·63	·54	·0171	·0156	·0174
	1·00	·90	·80	·70	·60	·0191	·0174	·0194

All radii and projections are given in centimetres. On the top row are the distances of the inner edge of the strip from the centre of the disc: the columns below show the amounts of projection used at these distances; in the last three columns are the 'upper and lower difference-ratios' obtained under these conditions. Column *A* shows the ratio of the difference of intensities of background and of the darkest part of the ring (MR , fig. 6) to the intensity of the background. Column *B* shows the ratio of the difference of intensities of background and of the lightest part of the ring (VQ , fig. 6) to the intensity of the background. Column *C* shows the ratio of the difference of intensities of background and of the darkest part of the ring to the intensity of the darkest part of the ring (MR , fig. 6).

The principal advantages of this apparatus are the accuracy with which the intensities of the light can be measured, the possibility of varying the position of the ring, and the elimination of the inhibitory effect of a dark background by placing the grey to be detected in the centre of a large area of light.

The micrometer screw-gauge used for measuring the extent to which the strip projects beyond the edge of the sector reads to 0.01 mm. As the amount of projection used in these experiments varies from

0.06 cm. (with radius 6 cms.) to 1 cm. (with radius 10 cms.), if an error of 0.01 mm. (a very large and unnecessary one) be made in reading the screw-gauge, and if an error of 0.5 mm. be made in adjusting the projecting strip to the lines on the sector marking points 6, 7, 8, 9, and 10 cms. from the centre, the largest possible error in the values of the difference-ratios shown in Table I will be about 0.00009. With moderately careful experimenting the values shown in the table are, therefore, practically correct to the fourth decimal place: the fourth figure cannot be more than one unit wrong.

The advantage of varying the size of the ring is so patent that little need be said about it. In these experiments the moveable strip was placed in irregular order in one of five positions, viz. 6, 7, 8, 9, or 10 cms. from the centre of the disc. The subject was not told that it would be placed exactly at these points, nor did he know whether a plain disc might be shown; so that even if he did not see the ring, but guessed its position, the chance of his guessing it correctly was very small. Therefore, when he located it correctly, it was probably because of some perceived darkening of the disc produced by the projecting strip.

The advantage of placing the grey to be detected in the centre of a large area of light is well brought out by this apparatus. Along the outer edge of the disc there is a perceptible brightening of the field due to contrast with the dark background: it extends also along those parts of the disc adjacent to the projecting bar which carries the axle on which the sector rotates. This brightening has an interesting effect on the appearance of the ring. All the observers remarked that when the ring was very faint it could be seen most easily on the left side of the field, and that unless it was very clear it could not be seen on that portion of the disc which is adjacent to the bar. At first they generally tried to find it on the right for the purpose of locating it correctly, but on failing to discover it there they explored the whole of the disc and often saw it on the left. For this reason after a few observations had been made most observers looked first at the left half of the field, and, on discovering the ring there, tried to follow it round to the right in order to find its exact position. Now, on the left, there is a very large area of light, while on the right the disc is crossed by the projecting bar. The blackness of this bar stands out in marked contrast to the light grey of the disc, and, not only does it make the adjacent parts of the disc brighter than the rest, but apparently it exercises a strong inhibitory influence on the ring, making it less clearly visible there than anywhere else. One observer remarked that the ring seemed to

pass behind a screen before it reached the scale on the cross-bar, so that it appeared to end about half-an-inch above and below it. That this effect was not due to any irregularities in the milk-glass screen was proved by reversing the screen, so that the part which formerly stood on the left was put on the right; under these conditions the phenomenon still remained. Nor was it due to not placing the source of light on the line running through the eye of the subject and the centre of the disc, for the position of the lamp was carefully measured before each series of observations was made.

An investigation of the exact extent of this inhibitory effect and an attempt to explain it are unnecessary here and would unduly complicate the subject of this paper; but, however the phenomenon may be explained, it is one which must be reckoned with. In these experiments it was allowed for by placing the projecting strip at points from 6 to 10 cms. from the centre of the disc, so that there was always a space of not less than 2 cms. between the outer edge of the ring and the outer edge of the disc, and a space of not less than 1 cm. between the inner edge of the ring and the outer edge of the very faint ring formed by that portion of the screw which projected beyond the counterpoise.

II. METHOD OF CONDUCTING THE EXPERIMENTS.

As the object of this investigation was qualitative as well as quantitative, being the discovery and explanation of any differences there might be between unocular and binocular discrimination of brightnesses, full introspection was demanded of the subjects; and as the dictating and writing of these introspections took some time, only a few readings could be taken at each sitting, viz., about 20 per hour. For this reason and because the time of both experimenter and subjects was somewhat limited, it has been impossible to make the large number of observations required in using the method of constant stimuli,—the method which is generally used in investigations on sensory discrimination. Only a comparatively small number of short series has been made with each subject, but as each observation was made with the utmost care and described in great detail, it is hoped that the fulness of the introspections will throw more light on the problem than the mere mathematical treatment of thousands of affirmative and negative judgments.

The method of conducting the experiments was as follows:

The approximate position of the threshold of discrimination was found by showing a few rings of different intensities. Then series of

projections were selected so that they gave eight or nine rings, the intensities of which formed an arithmetical series, those of the highest intensities being imperceptible and those of the lowest clearly perceptible. These rings might be of any radius from 6 to 10 cms. In each series the radii of the rings were varied in irregular order, care being taken, however, that as far as possible rings of each intensity appeared as often in one position as in another; so that the subject had no means of finding the position of a ring other than from the visual presentation.

With three subjects the intensities in each series were gradually decreased or increased. When this method,—a form of the method of minimal changes—was used, the length of the series was varied from time to time so that the subject should not learn to expect a change of judgment at any particular point in the series. One advantage of this method is that comparisons are avoided which are often made involuntarily when large and small differences are presented in irregular order. Such comparison tends to increase the effect of large intensity-differences and to decrease that of smaller ones. The gradual decrease or increase makes possible an adaptation which, though perhaps not entirely sensory, is comparable in its effects to the well-known phenomena of adaptation to bright and dim illumination.

To one of the subjects the different intensities were presented in irregular order, but in such a way that each was given approximately as often first as second, third, etc., and as often of one radius as of the others. This is simply the method of constant stimuli. More observations were made by this subject than by the others. The order of presentation of the stimuli in all the series was determined after the preliminary series had been made in which the approximate threshold was found.

The objection which is usually raised against the method of minimal changes, viz., that the subject's judgments are influenced and in some cases entirely determined by the fact that he knows that the stimuli are decreasing or increasing regularly in intensity, cannot be raised against the modification of this method which has been used here. At each observation the position of the ring has to be found, and the knowledge that its intensity is regularly increasing or decreasing can do nothing more than prepare the subject for the *kind* of ring to look for. He was not told how the intensities would vary and, although he soon discovered that they were decreasing or increasing, he thought that the change was somewhat irregular. This was due partly to the limited range of the intensities and partly to the necessity of giving

a full account of what had been seen, which tended to distract the attention from everything but the observation of the moment, and so to some extent prevented side-comparisons and reflection on the method of varying the stimuli. The general character of the judgments given in the regular and the irregular series was the same.

The series were arranged so that the subject looked alternately with both eyes and with one: when a ring of a given intensity had been observed with both eyes, another, of different diameter but like physical intensity, was observed with one eye (always the better one); then two other rings of slightly less intensity were similarly observed, and so the series was worked through. In this way the effects of practice, fatigue, and other varying influences were equally distributed over the uniocular and the binocular series, and a point to point comparison between them became possible.

It might be urged against the alternation of uniocular and binocular observations that it might be distracting to the subject. The introspective records, however, show no evidence of this, and the subjects were not aware of any distraction that could be attributed to this cause. If such distraction existed, it would affect both series equally, and would therefore be negligible.

The subject was directed to look at the disc when he heard the signal 'Now,' and then to look for the ring and find its position as accurately as possible by means of the teeth on the bar which stretched half-way across the disc, the distance of each tooth from the centre of the disc having previously been learned. He was asked to give a signal when first he saw the ring, and to note its appearance as carefully as possible. The experimenter fixed the projecting strip in the required position and set the sector rotating. When it was rotating quickly enough two signals were given, viz., 'Ready' and 'Now,' separated by an interval of about two seconds. At the same time that the second signal was given the experimenter set in motion the seconds-hands of a stop-watch, by which he measured the interval that elapsed before the signal was given which indicated that the ring had been seen. Thirty seconds were allowed for each observation: at the end of that time the experimenter called out 'Stop'; whereupon the subject ceased to look at the disc and began to describe what he had seen. He continued to examine the disc during the whole of the thirty seconds, whether the ring had been located or not. This interval was chosen because it allows sufficient time for searching the disc, and is not long enough to produce any strain.

During the first few observations the subject was only instructed to locate the ring and to make what remarks he could about its appearance; but, as the work proceeded, his introspection became more detailed and his attention was directed to the following questions:—

- When did you first see the ring, or any part of it?
- Did you see it continuously, or did it fluctuate?
- Did you see the whole of the ring at any one time? How often?
- Could you see any part of it you fixated?
- Was it clearly marked? Were both edges distinct?
- Where did you see it first?
- Where did you see it most easily?
- How did it first appear? Did it stand out suddenly and involuntarily, or did it gradually develop as you looked at it?
- Are you sure you saw the ring, or part of it?
- Are you sure you have located it correctly? Why?

He was allowed to give his introspection as he pleased; no attempt was made to direct it by requiring an answer to each of these questions in turn. In this way it was hoped to bring out the parts of each experience which struck him most forcibly. Occasionally after he had given as full an account as he could, he was asked one or two of these questions. On rare occasions all the questions were read through before beginning a series: further direction of attention to them was found unnecessary. The subject's attention was not directed from the first to the above questions because that would have interfered seriously with his observations: for him the most important part of the observation was the detection and location of the ring, and it was considered advisable in the first few experiments to let him attend specially to that, and afterwards, as he became practised, to encourage a more and more complete analysis of his presentations.

The subject sat at a constant distance (120 cms.) from the screen, and the lamp which illuminated it was placed at the same distance from it on the opposite side.

Each series lasted from three-quarters of an hour to an hour, according to the length of the introspection. Usually only one series was given at each sitting, but sometimes, when the introspections were short, two were given. Generally, the hour at which the observations were made was the same for the same subject. In two cases two sittings were given per week, in the others more; with all subjects the interval that elapsed between two consecutive series varied.

The experiments were performed with four subjects, Messrs Paul, Anderson, Robieson, and Craig, all graduates who had had considerable

experience in psychological observation. To these gentlemen I am deeply indebted for the time and care given in making these observations, and to Dr Watt for his invaluable advice and assistance.

III. TABULATION OF RESULTS.

The introspective records vary considerably in fulness of detail; they invariably increase in length as the subject gains experience in the task before him: as a rule, they are shortest when the ring is very clear and when it is not seen. The following are typical examples:—

Subject, R. Inner radius of ring, 6 cms. Diff.-ratio, 0·0095. Both eyes. "When I called out (after 6 seconds) I thought I saw a circle about the middle of the left side of the disc. It was very vague and indistinct, and I was not at all sure that it was the circle but fixation made it clearer. I could follow it round over the top, but with considerable difficulty, and, so far as I could make out, the inner edge was at 6. I could see it only by careful fixation, and even then only vaguely: still, I am practically certain it was the circle. I could see only very little of it at a time."

Subject, P. Radius, 8 cms. Diff.-ratio, 0·0076. Right eye. (Signal given after 17 seconds.) "At 8 there appeared a very indistinct darkening about twice the usual breadth of the ring, in the left-hand quadrant towards the top. I was not at all sure of it, and did not think I would make anything of it, but a part of it at the top of the disc defined itself and I saw the inner edge, then a part to the right of that."

Subject, A. Radius, 7 cms. Diff.-ratio, 0·0076. Both eyes. (Signal given after 6 seconds.) "The ring was at 7. It was seen first just above the centre. I was able to trace it round in the upper part of the disc. It was not quite so dark as it was the last time I used both eyes. I did not see the edges very easily or very clearly, but it was quite permanent, and I could trace it round continuously. I am quite sure it was the ring."

The records of any one series of observations, either binocular or uniocular, show that as the difference between the intensities of the background and the ring is gradually decreased, there are well-defined changes (i) in the appearance of the ring, (ii) in the accuracy of locating it, (iii) in the interval which elapses before it is first seen, and (iv) in the degree of certainty with which the judgments are made. When this difference is appreciably above the threshold of discrimination, the whole of the ring is seen simultaneously with its edges clearly defined; it remains steadily in view; there is no difficulty in finding its exact position; and it is seen at once. When the difference is less, the parts of the ring that are not in the centre of the field of vision often vanish and sometimes even a part that is in the focus of vision disappears; the edges are not well-defined; the reaction-times are slightly longer; and,

though the location is generally correct, the judgments are given with greater hesitation. With still smaller intensity-differences steady fixation is necessary before the ring is seen at all, and then only a small part is seen at a time; it is generally seen first on the left or at the top of the disc; and to find its position it is necessary to follow it round gradually to the scale, fixating parts of the disc at the same distance from the centre as the part seen; often only the approximate position is given; the reaction-time is comparatively long; and the subject is generally more certain of having seen the ring than of having located it correctly. Finally, with the smallest differences, the ring, if seen at all, is seen only in 'glimpses,' *i.e.* it appears for an instant and then disappears. At this stage there is very great difficulty in detecting whether what is seen is subjective or objective. As it is almost impossible to follow the ring round to the scale in the manner described above, its position has to be found by comparing its distance from the outer edge of the disc or from the outer edge of the ring formed by the rotating counterpoise with the distance of the teeth from those parts of the disc; the location is, therefore, generally only approximate. The reaction-times are very long, and the judgments are given with the greatest hesitation. It sometimes happens that a subject will say he has not seen the ring, and will then go on to say that he *thought* he had a glimpse of it, the correctness of the location showing that he probably did see it.

Correctness and Certainty of Location. In Table II is shown the frequency with which rings of each intensity were correctly located by each subject. Table III shows the number of times each ring was located correctly and with certainty. From both tables are excluded those observations in which rings were seen at several places: this is necessary because little importance can be attached to observations in which rings are located at two or three different distances from the centre of the disc.

These tables show that with binocular observation the ring was correctly located more frequently, and the subjects were more frequently certain that their localisations were correct. They show, too, that the lowest intensity-differences which evoke correct locations are very nearly the same in the binocular and the uniocular series, and that the range of intensities over which the percentage of correct locations falls from 100 to 0 is smaller in the former series than in the latter. On comparing Tables II and III it will be seen that the number of 'correct and certain' binocular locations is greater than the number of 'correct' uniocular locations.

*Discrimination of Brightness*TABLE II. *Table showing the number of times each ring was located correctly and in one position only.*

Intensity	P.		A.		R.		C.	
	Both	Right	Both	Left	Both	Right	Both	Right
0·0191					7	6	10	10
0·0171	22	22			7	6	10	8
0·0152	22	22	11	11	7	4	10	9
0·0133	22	22	10	10	6	5	10	8
0·0114	19	19	11	8	6	3	10	7
0·0095	21	11	11	9	4	1	9	1
0·0076	17	7	10	7	3	—	4	2
0·0057	10	4	7	2	—	—	3	1
0·0038	3	—	3	3	1	—	2	—
0·0019			—	—				
No. of series	22		11		7		10	

TABLE III. *Table showing the number of times each ring was located correctly and with certainty.*

Intensity	P.		A.		R.		C.	
	Both	Right	Both	Left	Both	Right	Both	Right
0·0191					7	4	10	10
0·0171	22	22			7	2	10	8
0·0152	22	22	11	10	6	1	10	9
0·0133	22	21	10	9	5	2	10	7
0·0114	19	18	11	7	1	—	9	1
0·0095	21	8	10	4	3	—	6	—
0·0076	12	5	8	1	—	—	2	—
0·0057	2	2	5	—	—	—	—	—
0·0038	—	—	1	2	—	—	—	—
0·0019			—	—				
No. of series	22		11		7		10	

It is worth noting that, with few exceptions, the subjectively certain locations are objectively correct. Of the 834 observations made in these experiments there were only six in which the location was wrong, while the subject was certain that he had seen it and located it correctly: this is 4·8% of the total number of observations in which the ring was located incorrectly—a very small proportion.

The *amount* of the difference between binocular and uniocular discrimination can be measured only very roughly, and will vary with the method of measurement. A rough measure is given by the difference between the intensities at which 50% of correct locations (or correct and certain locations) were made. These intensities are:—

Subject	Correct		Correct and certain	
	Binocular	Uniocular	Binocular	Uniocular
P.	0.0060	0.0095	0.0074	0.0101
A.	0.0050	0.0070	0.0060	0.0105
R.	0.0086	0.0119	0.0126	0.0186
C.	0.0080	0.0108	0.0090	0.0127

The difference-ratio, then, which evokes 50% of correct locations is about half as much again when only one eye is used. At frequency 75% correct the difference is greater still.

It ought to be noticed here that inaccuracy of location is not always a proof that no appreciable effect in consciousness has been produced by the projecting strip on the rotating sector. The task of locating the ring is more complicated than one would expect. When the ring is clear and well-marked there is no mistake except occasionally in the first few series, when a subject may forget the numbers of the teeth on the scale or when his attention is so much concentrated on examining the ring that he either forgets to locate it or, having done so, forgets afterwards where he has located it. But when it is less clear, errors are frequently made in cases where there is reason to believe that the ring has been detected. It is frequently remarked that when the ring is seen only in detached pieces the position of a piece on the left is different from that of one on the right. A small piece of the curve seems farther out than a larger piece. Probably this is a form of the well-known illusion that when two arcs of the same circle are of different sizes, the smaller seems to be part of a larger circle. This would account for a good many of the errors of localisation. Such introspections as the following support this explanation:—

“I was surprised when it did fall on the sixth notch: I had thought it was a little farther out.” (It was at the sixth.)

“At first I thought it was at the eighth notch, but decided it was at the seventh. I saw it first at the top of the disc.” (It was at the seventh.)

“At the eighth. It appeared on the extreme left of the disc. I find I have not been allowing enough curvature.” (It was at the eighth.)

On the other hand, when the part seen is above the scale of notches the location is usually accurate, and the subject is certain he has located it accurately.

When the ring could be detected only after careful and continued fixation of various parts of the disc, the difficulty of finding its position was very great, for any attempt to do so sometimes only led to its total disappearance.

When only one glimpse was obtained of a small part not near the scale, its position had to be estimated after it had disappeared by its distance from the centre or from the outer edge of the disc, so that the process of locating the ring near the scale was different from that of locating it far from the scale.

Sometimes the position of the ring was found by noting its position with regard to the inner and outer bright rings. Sometimes it was known by the tendency to confuse it with the ring formed by the projecting screw; thus one subject said: "I think it was at 7, because it was near the centre but did not tend to get confused with the circle round the counterpoise as a ring at the sixth notch does." (It was at 7.)

The localisation is only approximate when the ring is first seen late in the experiment, or when it is not well defined, *e.g.* :

"As I gave the signal, the grey ring appeared about the top of the disc. I had not time to refer it to the scale. It might be at 8 or 9." (It was at 9.)

"A greyness about twice the breadth of the ring seemed to stretch all the way round. It was at the seventh or eighth notch; I could not localise it definitely because it was twice the breadth of the true ring. The inner edge would be about 7." (It was at 7.)

There can be little doubt, then, that the ring was sometimes seen but not correctly located; but it is improbable that in such cases the location was more than one centimetre from the correct position. It might seem advisable, therefore, if we wish to find how often the ring was seen, to include with the correct locations those observations in which the location was not more than one centimetre incorrect. Such a procedure would, however, almost certainly lead to the inclusion of subjective impressions, *i.e.* impressions not directly due to the objective ring on the disc, and as the probability of hitting by chance on the correct or nearly correct positions is very much greater than that of hitting only on the correct position, it seems better to base our conclusions on those observations in which the ring was located correctly.

Reaction-times. Another interesting difference between binocular and unocular discrimination is in reaction-times. The subjects were instructed to give a signal as soon as they saw the ring, and the interval between the beginning of the observation and the giving of the signal was measured by means of a stop-watch. The averages of the reaction-times with their mean variations are shown in Table IV. In compiling this table only those observations have been included in which the location was correct or within one centimetre of the correct position.

TABLE IV. *Reaction-times (in seconds).*

Intensity	P.						A.					
	Both eyes			Right eye			Both eyes			Left eye		
	No.	Av.	M.V.	No.	Av.	M.V.	No.	Av.	M.V.	No.	Av.	M.V.
0.0171	22	3.7	1.7	22	7.3	4.3						
0.0152	22	4.3	1.4	22	8.1	4.3	9	2.1	0.9	9	4.0	1.3
0.0133	22	4.3	1.2	22	13.0	6.1	9	2.9	1.4	9	8.6	3.6
0.0114	21	7.2	3.8	19	15.9	5.3	9	4.2	1.3	7	14.8	6.3
0.0095	22	9.6	5.4	14	23.7	4.6	9	8.7	5.1	9	17.3	8.3
0.0076	17	13.8	6.7	7	18.1	7.9	9	10.9	6.5	8	21.4	6.1
0.0057	9	23.7	5.4	3	21.7	11.1	7	14.6	5.9	2	12.5	2.5
0.0038	—	—	—	—	—	—	5	21.6	7.3	3	12.8	7.6
0.0019	—	—	—	—	—	—	3	10.3	11.1	—	—	—

Intensity	R.						C.					
	Both eyes			Right eye			Both eyes			Right eye		
	No.	Av.	M.V.	No.	Av.	M.V.	No.	Av.	M.V.	No.	Av.	M.V.
0.0191	5	2.6	0.5	5	10.0	4.4	10	5.0	2.7	8	10.8	5.9
0.0171	5	3.1	1.5	4	7.7	2.4	9	5.2	1.6	9	9.5	3.9
0.0152	5	5.4	1.1	3	20.6	5.8	9	4.7	2.4	9	12.6	5.5
0.0133	5	5.8	3.4	4	13.7	2.8	10	8.8	4.2	8	12.5	6.8
0.0114	5	13.5	4.0	2	20.5	1.0	10	13.7	6.6	3	17.6	9.9
0.0095	4	9.7	2.1	1	27.5	—	6	12.3	4.2	1	30.0	—
0.0076	3	23.6	1.8	—	—	—	5	21.0	4.4	1	22.0	—
0.0057	—	—	—	—	—	—	—	—	—	—	—	—
0.0038	—	—	—	—	—	—	—	—	—	—	—	—

It will be seen that as the difference between the intensities of the ring and of the background decreases, the reaction-times increase, and

that the mean variation (which measures the variability of the individual reaction-times about their average) increases too. There is also a well-marked difference in the average reaction-times of the binocular and the uniocular series. If we exclude the lowest intensity-ratios, the average reaction-time in the uniocular series is about twice that in the other; if we exclude these, it is about half as much longer.

The instruction to give a signal as soon as the ring was seen seemed at first to be quite clear and definite, but later proved to be not sufficiently explicit when the ring was so faint that it was seen only in glimpses, for then it might mean "React as soon as you see any indication of the ring," or, "React as soon as you are sure you have seen it." No attempt was made to remove this ambiguity, for it was not discovered until the experiments were well advanced. This may account for some of the variability in the reaction-times, for it is quite apparent from the introspections that the instruction was interpreted sometimes in one way, sometimes in the other, even by the same subject.

IV. EXPLANATION OF RESULTS.

Apart from stereoscopic differences binocular and uniocular discrimination differs in several respects: small differences of intensity are detected more frequently when both eyes are used, judgments are given with more certainty and consistency, and their reaction-times are shorter. How are these differences to be explained?

The explanations which suggest themselves most readily are lack of practice in uniocular discrimination and the possibility of summation of brightnesses of the uniocular images. Neither of these, however, is satisfactory.

Practice. Practice, no doubt, accounts for the apparent improvement in both kinds of discrimination after the first series, but it does not account for the differences enumerated above, for if it did, since normal vision gives less practice in uniocular discrimination than in binocular, we should expect more rapid improvement in the former than in the latter, and there is no evidence of such improvement. In Table V is shown the total number of correct locations made by all four subjects during the first and second halves of the observations. In making this table the first series in A.'s and R.'s set have been omitted in order to make the number of series even. It will be seen that when all the observations are put together there is in each case a slight

increase in the number of correct locations in the second half of the experiments, the increase in the binocular observations is about 19 % while in the uniocular it is only 7 %. This difference is hardly large enough to be significant, but, such as it is, it is just the opposite of what one would expect if the suggested explanation were correct.

TABLE V.

	Both eyes		One eye	
	1st half	2nd half	1st half	2nd half
0.0152	24	24	22	23
0.0133	23	23	23	22
0.0114	20	24	18	18
0.0095	21	23	10	12
0.0076	13	21	7	9
0.0057	9	11	3	4
0.0038	2	7	1	2
Total	112	133	84	90
Increase	19 %		7 %	

Summation of Brightnesses. The other explanation, that the ring seen by both eyes is relatively darker than that seen by one because of a summation of the brightnesses of the uniocular images in the binocular, is even less satisfactory.

There is great difference of opinion as to whether an illuminated surface appears brighter in binocular vision than in uniocular. Valerius¹, Aubert², McDougall³ and others say it does. Valerius¹ says the amount of the difference is $\frac{1}{16}$ of the brightness of the uniocular image. Aubert⁴ puts it at from zero to $\frac{1}{16}$, and says there is no perceptible difference "with brightnesses greater than that of white paper in diffuse daylight indoors." McDougall⁵ thinks that "dimly illuminated surfaces appear brighter in binocular than in uniocular vision, but that with brightly illuminated surfaces no such difference is perceptible." Sherrington⁶ could detect no difference

¹ *Ann. d. Physik u. Chemie*, 1873, CL. 323.

² *Physiologie der Netzhaut*, 1865, 286.

³ *Brain*, 1910-11, XXXIII. 372.

⁵ *Loc. cit.*

⁴ *Physiologische Optik*, 1865, 500.

⁶ *This Journal*, 1904-5, I. 50.

within the limits of brightness with which he experimented. Even if there be any such difference, it cannot be invoked to explain the differences between binocular and uniocular discrimination, for we must suppose that all parts of the illuminated binocular field (ring and background) will be brighter than similar parts of the uniocular field by the same proportion, and consequently the ratio of the intensity of the ring to that of the background will be the same in both cases.

If the ratio of the difference of the brightnesses of the binocular and uniocular images to the brightness of one of them be not the same for all intensities of the stimulus, it will still be impossible to explain our phenomena by this means, for the brightness of the ring is so very nearly the same as that of its background that any such law of differential increment will be practically inoperative. What little evidence there is tells against this explanation. Aubert, Piper¹ and McDougall say that the increase of brightness of the binocular image over the uniocular is greater with dim illumination than with bright. This, if it has any differential effect at all, ought to make the binocular threshold of discrimination higher than the uniocular and not lower, as it really is, for binocular vision should increase the brightness of the ring more than that of the relatively brighter background.

Finally, the strongest objection to this explanation is that direct comparison shows no really satisfactory evidence of any difference of brightness-differences, and introspection is after all the final test. Such comparison was seldom invited but was frequently made. It shows that when the brightness-differences are well above the threshold of discrimination there is no apparent difference between the impression received by one eye and that received by two. It is difficult to see how this fact can be reconciled with the suggested explanation. When the brightness-differences are near the threshold, as they are in these experiments, there *is* a difference between the uniocular and the binocular images, but it is not a difference of brightness. My subjects after some practice were able to distinguish the grades of intensities with very great accuracy, so that although they sometimes said that a brightness-difference was equal to one which was really slightly less or slightly greater, they rarely said that it was greater than one which was really less than it (only twice, in fact). One would expect, therefore, that if any difference in the brightness-differences of the uniocular and binocular images existed, they would have been noticed. Yet only once did subject P. remark that a ring seen with both eyes was darker

¹ *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 1903, xxxii. 176.

than a similar one seen with one eye, and subjects R. and C. not at all. Subject A. noted a difference of brightness ten times, but it is possible that he may have confused steadiness with darkness of grey. It may be worth noting that it was he who on two occasions said that a grey was darker than one which was really brighter. With such small intensity-differences as were used here, the comparison of intensities is very difficult, and very little importance can be attached to the few cases in which a difference of brightnesses was recorded. Any uncertainty, whatever be its cause (length of time allowed for observing, intermittence of the impression, or lapse of attention), may be attributed to a decrease in the brightness-differences of the impression. Indeed it is a tribute to the care with which the observations were made that such confusion did not take place more frequently. Considering the smallness of the difference-ratios used and the great possibilities of error, there is, then, little reason for supposing that in the binocular image there is a summation of the brightness-differences of the uniocular ones.

Introspective basis for an explanation. In order to arrive at a satisfactory explanation it will be necessary to examine the detailed introspections of the subjects. It has already been remarked that as the difference between the intensities of the ring and the background decreases there are clearly marked changes in the appearance of the former which affect the accuracy and quickness of location, the interval that elapses before its first appearance, and the degree of certainty with which judgments are made. When the difference is above the threshold of discrimination the ring is seen continuously as a whole and there is no difficulty in locating it; when it is smaller the ring is seen in fragments which disappear and reappear, and on this account location is somewhat difficult; finally, when the smallest differences are used very steady fixation is necessary, and even then the ring, if seen at all, is seen in glimpses. At this last stage only very small portions of the ring are seen at a time; sometimes they are fairly clear, sometimes they take the form of an undefined blur often twice as broad as they should be, sometimes only one edge is seen and sometimes the edges are seen alternately. There is consequently great difficulty in deciding whether what is seen is objective or subjective. (I shall restrict the term 'objective' to impressions of rings which are formed by rotation of the projecting strip on the sector.) These faint objective impressions appear to differ very little from others which are certainly subjective, as the absence of any objective cause for them

in the rotating disc shows. Here are a few examples of purely subjective impressions:—

Radius 9. Diff.-ratio, 0.0038. Signal given after 30 seconds. "At the sixth notch. Just as you said 'Stop' it appeared quite clearly and suddenly at the top of the disc. Its edges were well defined. I saw it only for a very short time, and I saw only a very small part of it. It appeared exactly like the ring; had well defined edges. It seemed to appear without any effort of attention. It was the proper breadth, and looked just like a part of the ring when it is very clear. First when I looked there was a greyness at the ninth notch, but it never assumed the form of a ring."

Radius 6. Diff.-ratio, 0.0095. "There was a greyness about the eighth notch which looked like the ring and became clearer towards the end of the observation."

A blank disc was shown. "When I gave the signal (after 3 seconds) I saw the ring as a whole. It was rather indistinct. It seemed to be near the eighth notch. I saw it constantly throughout, but I was not always certain of the location. When I looked near the scale it seemed to be at the eighth notch, but when I had only the left-hand side in view it seemed to be closer in. I am quite sure that it was at the eighth. The edges were never well defined. I did not see the ring at any other part of the disc."

Some of these impressions—those of rings of radius 6 and 10 cms.—may be due to contrast: the outer and inner edges of the disc were brighter than the rest of the field because of contrast with the dark background and the dark centre formed by rotating the counterpoise; the parts of the disc just inside the large bright ring and outside the small one may have been slightly darker than the rest of the field because of contrast with these bright rings. This would explain the fact that rings were inaccurately located at 10 and 6 slightly more frequently than at any other position. Yet it does not explain all these impressions, for they were located almost as frequently at 7, 8 and 9. After-images of the outer edge of the disc may have produced some of them, but other causes must have been at work too.

What the causes of these subjective phenomena may be does not concern us now, but there can be no doubt of their existence¹. They are detected in the observations of all the subjects, although they are more common with some than with others. They are not the product of continued observation in this class of experiment, for they are present from the very first; nor are they due to carelessness. They are a source of considerable disturbance in the detection of very small differences of stimuli, so much so that a large part of the observation

¹ Cf. Oswald Külpe, "Ueber die Objectivirung und Subjectivirung von Sinneseindrücken," *Philos. Stud.*, 1902, xix. 508.

seems to be given up to deciding whether what is seen is objective or not. The following records show this very clearly :

Radius 8. Diff.-ratio, 0.0038. Signal given after 27 seconds. "At the sixth notch there appeared a ring half as broad as it should be, with its inner edge well defined. It appeared above the centre of the disc. I did not take it to the scale but I could see that it was at the sixth. It was an arc of about 60° with no outer edge: it just shaded off into the background. There were two other rings, one at the eighth and one at the tenth. I should place the eighth ring next the sixth one, but it was only a greyness; one part of it about an inch long appeared there for an instant, but it disappeared so suddenly that I thought it was not the ring. The inner ring was much too narrow, and the outer one much too broad; it was twice the breadth of the proper ring; the edges of the outer ring faded off. I cannot say which was the ring. The middle one appeared first, then the outer one, then the inner one."

Radius 6. Diff.-ratio, 0.0095. Signal given after 25 seconds. "A very indistinct ring at the sixth notch. It was broader than usual by about one-half. There were also other greynesses at about the eighth or tenth, but their edges were not so well defined."

The reports of my experiments give some indication of the standards by which the subjects decide whether their impressions are objective. Several criteria are used, sometimes singly, sometimes together. The chief are permanence of the impression, ability to detect various parts of the ring, its curvature and breadth, clearness of outline, relation to fixation and attention, and absence of similar impressions elsewhere. A part of the ring that appears only for an instant or that vanishes while it is being fixated and does not return is usually classed as subjective. On the other hand, if it can be seen at other parts of the disc, or, if it stands out steadily for some time, it is supposed to be objective. A large part is more likely to be considered objective than a small one. The following extracts from the reports of observations in which the ring was located correctly will show the importance of these considerations :

"It appeared first on the left, then it disappeared and I thought it was not the ring, but it reappeared and I became sure."

"After giving the signal I thought I had made a mistake, but it returned."

"I did not see the ring. Once, about 8 cms. from the centre and above it, a small part of what might have been a ring appeared, but it was just for an instant." (There was a ring at 6.)

"A greyness about 7 cms. above the centre, but I should not care to say it was the ring. It seemed a little broader than it should be. It appeared about the middle of the observation. It never became defined. Another time I saw a greyness about 11 or 12, but it was not so persistent as this." (It was at 7.)

"I could not tell at first whether it was the ring, but when I saw it at three parts of the disc all the same distance from the centre, I became sure it was."

"I was not at first sure it was the ring and did not give the signal till I had traced it round from left to right."

"I had glimpses of what may have been rings at 6 and 11, but that at 6 was more permanent and I seemed to see more of it. I think it was at 6." (It was.)

When the edges are blurred and hazy there is often considerable doubt about its objectivity; if, however, they are or become clear, the subject is fairly sure that he has seen the ring. Sometimes rings were seen that were broader or narrower than usual, or not quite circular; in such cases there was always some doubt as to their objectivity. When after searching the whole of the disc, only one ring could be found, the absence of other impressions would lead the subject to decide in favour of this one. On the other hand, after concentrating attention on an undefined blur to the neglect of the rest of the field, he was often doubtful about the objectivity of this impression both because of its indefiniteness and because of not looking elsewhere. A few more extracts will make this clear:

"I saw a greyness about twice the breadth the ring generally is, whose inner edge would be about 6 cms. from the centre. I should not like to say it was the ring, because I did not give the rest of the disc much attention."

"I saw two rings of greyness, one at 7, the other at 9. I think the ring itself was at 7, because its inner edge was more indistinct than that of the other. It was clearer and not so broad."

"There was a very indistinct ring at 6, but it was broader than it should have been by about one-half. There were also other greynesses about 8 and 10 but their edges were not so well defined." (It was at 6.)

When there is any doubt about the objectivity of an impression, it is because some or all of these marks of objectivity are missing. This conclusion is corroborated by an examination of those cases in which the ring was located so far from its true position that the impression could not have been due to any differences in the intensity of the light; and at the same time it partially explains those illusions, for in these cases there is an illusion of objectivity just because the impression shows some of the above-named marks of objectivity. A few examples from the introspections will make this clear:

Radius, 10 cms. Signal given after 22 seconds. "I saw what I thought was the inside edge of the ring on the left of the disc. It persisted, then disappeared and I saw nothing else. It was so well defined that I am almost certain it was there. There was a faint suggestion of the rest of the ring. It would be about 8. Yes, I am sure of the location."

Radius, 9 cms. Signal given after 30 seconds. "At 6. Just as you called 'Stop' it appeared quite clearly and suddenly with its edges well defined. It would be closer in than 6; 5, I think. I saw only a very small piece of it—about a quarter of an inch of it. It seemed to appear without any effort of attention. It was the proper breadth and looked just like a part of the ring when it is very clear. I had explored along the vertical line above the centre. First when I looked there was a greyness at 9, but it never assumed the form of a ring."

There seems to be considerable difference between subjects in their attitude towards these criteria. Some appear to be satisfied with only one of these marks of objectivity, while others are not satisfied unless they detect several. Subject R., for example, sometimes looked for several, and because he could not find them he was doubtful of the objectivity of his impressions.

This difference of attitude may account for a characteristic difference between subjects. Some are frequently doubtful of the objectivity of impressions the correct location of which shows that they have been due to differences of intensity in the light, and when the intensity-ratio falls below a certain amount, they almost invariably state that they have not seen the ring. Others not only do not subjectify their impressions, but even obtain glimpses of rings the positions of which are so far from that of the ring formed by the rotation of the strip on the sector that they are obviously not objective: they even continue to see rings or parts of them when the strip is entirely behind the sector so that a uniformly bright disc is shown. These subjective impressions are sometimes described in as great detail as objective ones, and frequently they are believed to be objective. Subjects of this type seldom say they have seen no ring. Subject A. is a good example of this class, subject R. of the other; P. and C. are more mixed in type.

Suggested Explanation. The brief account that has just been given of the difficulties experienced in making these observations will suffice to show that the process of discrimination is fairly complex, involving in some cases a very careful examination of the shape, size, distinctness, brightness and steadiness of our impressions. It is to one of the causes of this complexity—namely, the intermittence of a presentation—that we are to look for the explanation we are seeking.

Oscillation seems to be a feature of our mental life. Visual presentations, in particular, are constantly fluctuating: the visual appearance of even a well-defined object will change if the latter be looked at very steadily; and, when the brightness of the object

differs very little from that of the rest of the field (as in the greys used in these experiments) these fluctuations are very frequent and well marked.

All my subjects agree that the most pronounced difference between the uniocular and binocular images evoked by the same stimuli is in steadiness. A just discernible ring as seen by one eye was more intermittent than a similar one seen by both; it was sometimes described as having the elusive, fluctuating character of the clouds of light that are seen when the eyes are closed and the eyeballs pressed, or as being hazy, filmy and ghost-like; it was seen and located less easily, the edges were sometimes less distinct, and less of it was seen at once. Frequently it was remarked that the uniocular observations were more difficult to make than the binocular, and more careful observation showed that this difficulty was due to the greater fluctuation of the uniocular ring. Now it is evident from what has been said about the criteria of objectivity that an increase in the steadiness of an impression will produce an increase in the number of recorded objective impressions, and in the number of 'correct and certain' locations; hence if we can account for the above-mentioned difference in steadiness, we shall have the explanation we are seeking.

This explanation is, I suggest, to be found in the way in which the sensations evoked by the stimulation of each eye are integrated in binocular vision and is as follows: the sensations due to the stimulation of each retina are developed and fluctuate independently, their periods of fluctuation are not synchronous, and therefore in binocular vision there is an overlapping of the periods of appearance of 'corresponding' parts of the two images, and hence a relative increase in the steadiness of the impression.

Let us consider in turn each of the premisses on which this argument rests.

Sherrington and McDougall have called attention to some facts of visual experience which are explicable only on the assumption that the cerebro-retinal mechanisms of the eyes are independent. Sherrington found that when corresponding points of the retinae are stimulated by intermittent light and dark stimuli either synchronously or asynchronously the rapidity of alternation of stimuli required to extinguish binocular flicker is just the same as that required to extinguish uniocular flicker. He concludes that these experiments "show that during binocular regard of an objective image each uniocular

mechanism develops independently—at least as to steadiness of brightness and intensity of brightness—a sensual image of considerable completeness. The singleness of the binocular perception results from the combining of these elaborated uniocular sensations: it is the product therefore of a psychical synthesis that works with already elaborated sensations contemporaneously proceeding¹. McDougall has adduced other observations which are most readily explained in the same way.

Whatever be the nature of the physiological processes which accompany binocular experience, there certainly are features of that experience which indicate that even in many so-called cases of binocular fusion the uniocular images retain—at any rate as regards steadiness and brightness—the characteristics they have when they are present singly. An analogy may help to make my meaning clear. The sensations of sight and sound are to some extent independent: they are evoked by the stimulation of different sense-organs, they are independently variable and, although they may fuse to a greater or less degree or may partially inhibit one another, yet they are each capable of analysis from the rest of experience and each on analysis has the same attributes as when the other is absent. So it is with uniocular images: each appears to be evoked by the excitation of different cerebro-retinal paths which do not terminate in a common cerebral centre, and each is capable of analysis in binocular perception; although they fuse more intimately than do modally different sensations, yet they do not lose their identity in the resultant binocular experience: each preserves the attributes it has when the other is absent. I do not mean that the reciprocal influence of one image on the other is impossible; the facts of binocular rivalry disprove that. I mean merely that each image is relatively complete in itself and that the mode of integration of these images is a problem for the psychologist.

This theory appears to be the only one which can adequately account for Sherrington's and McDougall's observations and for all the observations described in the preceding pages. It receives further support from observations made to establish the second premise—that the right- and left-eye images fluctuate independently and asynchronously.

After the completion of the experiments already described, another series of observations was made in which the periods of intermittence

¹ *Loc. cit.*

of uniocular and binocular images were measured. In these experiments the subjects were instructed to press a reaction-key when they saw the ring and release it when it disappeared. The movements of the key were recorded on a rotating drum. Each observation lasted about 75 seconds, and each sitting began with one or more practice series. To facilitate the task of the subject he was directed to confine his attention to the part of the disc directly above the centre and to record the fluctuations of the ring there. To simplify the work still more, all the rings used in these experiments were of the same radius, and this was made known to the subjects before the experiments began. Such a procedure would be very unreliable with unpractised subjects, as their judgments might be very much influenced by the knowledge of the position of the ring, but with practised subjects the danger is not so great.

These observations are not easy to make, for it is difficult sometimes to know whether anything is seen or not, and the reaction occasionally distracts the attention from the disc. Some observers thought they discovered a tendency to tap rhythmically which determined the times of appearance and disappearance, but this did not often happen, and the introspections were not always corroborated by the tracings: if such a tendency did exist, there is no reason for believing that it seriously disturbed the experiments. There is another difficulty arising from the degrees of fluctuation. Sometimes the ring in the area under examination would disappear entirely, leaving a uniformly white field: sometimes different parts would alternately disappear and reappear. The subjects were instructed not to record the latter kind of fluctuations. This instruction may not have been carried out in every case, but after some practice all the subjects felt that they could keep their attention fairly constant: if any partial disappearances were recorded they were not numerous enough to affect the results seriously. Other disturbing conditions were a tendency to look for the ring on other parts of the disc, and the difficulty of pressing and releasing the reaction-key at the right moment. Yet, in spite of all these difficulties, the records are probably significant, for all the subjects were well practised, every possible precaution was taken, the observations were repeated many times, and the records agree in general tendency, although the subjects themselves during the experiments had no idea of the relative lengths of the periods of intermission.

Table VI gives a specimen record. The figures in the columns

show the intervals (in seconds) during which the ring was reported to have been seen or not seen. Each series began with an interval during which it was not seen and ends with one during which it was seen. This method of dealing with the results seemed better than that of allowing a fixed time for observation and stopping exactly at the end of it, because it made easier a comparison of the intervals during which it was seen with those during which it was not seen.

TABLE VI. *Subject P.*

Diff.-ratio	0.0133		0.0095		0.0057	
	Not seen	Seen	Not seen	Seen	Not seen	Seen
Right eye	5.2	4.7	11.8	1.2	32.5	2.0
	4.4	7.2	8.1	3.6	14.2	0.8
	6.8	2.1	3.3	1.1	11.8	2.8
	6.9	12.1	9.1	2.5	15.7	2.4
	6.0	2.8	7.1	7.0		
	11.4	5.6	7.6	2.1		
	7.7	0.9	1.8	3.6		
			10.5	3.0		
	48.4	35.4	59.3	24.1	74.2	8.0
Both eyes	5.0	17.8	5.3	8.4	7.9	1.1
	4.4	3.1	5.8	5.1	3.6	2.4
	3.4	5.7	5.2	4.1	2.3	2.8
	5.3	17.6	10.5	8.2	4.9	6.4
	6.8	1.9	7.3	8.0	7.1	6.0
	4.2	9.7	7.5	7.0	6.3	2.9
	2.8	0.7			8.6	1.5
					4.1	10.1
	32.3	56.5	41.6	40.8	44.8	33.2

This record, like all the others, shows marked irregularity in the times of appearance and disappearance in both the unocular and the binocular observations: the ring may be seen for a fraction of a second, disappear, and then reappear to remain for five or ten seconds more. The only point of resemblance between successive series of observations made with the same eye on the same ring is in the ratio of the total time during which it is seen to the time during which it is not seen; this remains fairly constant. In other respects they differ: the interval that elapses before the ring first makes its appearance and the time it remains before disappearing again vary considerably.

It cannot be demonstrated by direct observation that the right- and left-eye images fluctuate independently; we must therefore look for indirect evidence. This is to be found in a comparison of the rates of fluctuation of binocular and unocular images. If unocular images fluctuate independently one must suppose that sometimes the ring will be seen by one eye, sometimes by the other, sometimes by both and sometimes not at all. The time the ring is seen when both eyes are used will, therefore, be relatively longer than when only one is used. Observation shows this to be the case.

In Table VII is shown for each subject and each intensity the ratio of the total time during which the ring was seen to the total time

TABLE VII. *Table showing ratio of intervals during which ring was seen to intervals during which it was not seen.*

Intensity	P.		R.		A.		C.	
	Both	Right	Both	Right	Both	Left	Both	Right
0.0133	2.786	1.598	3.408	0.828	2.227	0.907	1.661	0.915
0.0095	1.535	0.624	2.133	0.610	1.734	0.635	0.991	0.124
0.0057	0.898	0.275	0.543	0.197	0.652	0.326	0.178	0.032

during which it was not seen. This ratio is in every case greater with binocular than with unocular observation. It also decreases as the intensity-ratio decreases.

Here it might be asked: even if the unocular images be developed independently and fluctuate asynchronously, what reason is there for supposing that in binocular vision the overlapping of the times of appearance of the ring in each image has the effect of increasing the relative interval during which it is seen? Is it not as reasonable to suppose that rivalry takes place so that sometimes the blank field seen by one eye inhibits the ring seen by the other? This, certainly, is logically possible, just as possible as the account we are giving; it does not, however, square with the facts. In the first place the phenomena described above are not conditioned in the same way as rivalry. Binocular rivalry is produced by presenting to the right and left eyes objects which differ very considerably in form or colour or both and evoke sensations which are equally different. In our observations the stimuli are alike, but the sensations sometimes differ slightly. In the first case the difference of sensation is due to differences in the stimuli;

in the second it is due to the oscillatory character of the psycho-physical processes. It would be unjustifiable to suppose that, because rivalry sometimes (not always) occurs between sensations evoked by very different stimuli, it will therefore occur between slightly different sensations evoked by the same stimuli. Again, there is seldom rivalry when one eye is stimulated by a blank field and the other by objects with well-marked contours, as when a blank card and one on which a letter is printed are seen in the stereoscope. It is only with very steady fixation that the letter will disappear; and even then it is doubtful whether the disappearance is due to rivalry or merely to the well-known fact that steady fixation produces fluctuations in any visual presentation. There is, therefore, no reason for supposing that the absence of a distinguishable ring in one retinal image inhibits its appearance in the other.

If our account of binocular perception be correct, if visual images fluctuate, if corresponding elements of the right- and left-eye images do not always appear synchronously, and if there is a consequent overlapping of the periods of their appearance which has the effect of increasing the interval during which each element in the binocular image is present in consciousness, we can account for all the observed facts—the frequency of correct locations, the certainty with which the judgments were given, and the reaction-times.

More negative judgments, *i.e.*, those to the effect that no ring was seen, were given in the uniocular observations because the ring being more intermittent there was a greater possibility of its being regarded as subjective. More correct locations were made in the binocular observations because, the impression being steadier, it was easier to find its exact position, and for the same reason more ‘certain locations’ were made in the binocular observations. The smallest differences of intensity that can be detected are nearly the same with binocular vision as with uniocular because the binocular image is an integration of the uniocular images and does not exist in their absence.

The reaction-times are similarly explained. It has been shown that these may measure either the interval that elapses before a ring is seen, or the interval that elapses before the subject is certain that he has seen the ring. The greater steadiness of the binocular image will, therefore, in a large number of experiments have the effect of reducing the reaction-time, for certainty comes more quickly with a steady impression than with one which comes and goes. The increase in the

mean variation of the reaction-times with increase of intensity-ratio is probably due to the greater difficulty of keeping one's attitude constant in reacting to impressions evoked by low difference-ratios; the difference between the mean variations of the binocular and the uniocular observations is to be explained in the same way.

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THE QUANTITATIVE INVESTIGATION OF HIGHER MENTAL PROCESSES.

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- I. *Objects of the Investigation.*
- II. *Historical.*
- III. *The Subjects and General Conditions of the Present Investigation.*
- IV. *The Tests and their application to the Subjects.*
- V. *Statistical Methods employed and Correlation Results.*
- VI. *Conclusions.*

I.

THE primary aim of this investigation¹ has been to ascertain to what extent different tests correlate with a subjective estimate of intelligence, and to select those tests which give the highest coefficients of correlation. For this purpose, the most satisfactory methods of previous investigators have been followed, refinements, however, upon earlier procedures being introduced whenever possible.

Indeed, the methods of applying mental tests to school children have left much to be desired. Too often the children have been subjected to distractions in the form of strange experimenters and unusual conditions of work. This division of the child's interests must unquestionably have affected the results obtained. Further, the actual application of the tests has often been placed in the hands of laboratory assistants who did not know the children, or in those of class teachers, persons usually untrained in psychological methods.

In the part of this research which was conducted at the Fielden Demonstration Schools, Manchester, the writer was the only supervisor

¹ It was conducted under the guidance of Mr T. H. Pear, to whom the writer is especially indebted for the advice and assistance so freely given.

present, and no one was allowed to enter or leave the room while the tests were in progress. The aim throughout was to secure the most favourable experimental conditions possible, so that the results would afford a reliable estimate of the mental traits under investigation. Consequently the conditions under which the tests were performed differ very considerably from those of most previous investigators.

Much uncertainty still exists regarding the hierarchical arrangement of the coefficients of correlation of each test with every other. At the present time opinion is about equally divided and it is hoped that the results of this investigation will assist in defining the correct view.

Recently the question of a 'general factor' underlying mental processes has been the subject of much discussion. Many investigators deny its existence altogether and those holding the opposite view are not agreed upon its constitution. It was hoped that evidence would be adduced here which would help to solve this problem.

Many of the tests in this research involve what may be termed 'general ability' rather than knowledge which has been acquired in school. Some of them bear no resemblance whatever to school work, and hence they afford a means of testing the real (in opposition to the apparent) intellectual capacities of the child.

II.

An excellent summary of the work done in this field is to be found in a book by William Brown¹. Since the publication of this book, Burt² has issued the results of a research on "Experimental Tests of Higher Mental Processes and their relation to General Intelligence." He concludes that "those tests involving higher mental processes such as Reasoning, vary most closely with Intelligence, and are least vitiated by variations with irrelevant conditions, such as Sex, Social Status, Training of the experimenter, and mass-measurement of numbers of children at once³."

More recently there has appeared a paper on "General Ability, its Existence and Nature⁴." The authors utilise all available data for the establishment of their conclusions; data collected both by supporters

¹ *The Essentials of Mental Measurement*. Cambridge, 1911, 81—97.

² *J. of exper. Ped.*, 1911, i. No. 2, 93—112.

³ *Op. cit.* p. 112.

⁴ Bernard Hart and C. Spearman: "General Ability, its Existence and Nature." *This Journal*, 1912, v. 51—84.

of their theory and by those antagonistic to it. In all these cases they consider that the 'general factor theory' is supported. They conclude that "the fact of correlation existing between quite different intellectual performances seems to be fundamentally identical with the fact that any such performance inhibits quite different simultaneous ones. Both phenomena are explicable by conceiving that every performance depends partly on some *common fund of energy*. This, then, is the required General Factor¹." Further, "every performance depends, not only upon this General Factor, but also in varying degree upon a factor specific to itself and all very similar performances²."

Simpson³, on the other hand, maintains that "there is no justification for the view that 'general intelligence' is to be explained on the basis of a hierarchy of mental functions," which are correlated owing to their common connexion with a central factor.

III.

The subjects who were examined during the present investigation comprised:

(a) A group of 34 children of both sexes (Group I) attending the Fielden Demonstration Schools, Manchester⁴. Their ages ranged from 11 to 13 years. In social status they were superior to the average elementary school-child. All had attended the school for several years; hence they had long been subjected to the same environmental influences so far as school-life was concerned. The tests were administered during the school hours of the mornings of Tuesdays, Thursdays, and Saturdays, the time occupied each morning by the tests being 40 minutes. The entire series of tests was given by the writer. Any distraction which, under ordinary circumstances, might have been caused by the presence of a stranger, was eliminated, for during the past two years the writer had frequently acted in the capacity of teacher to the same children. The school presented peculiar facilities for an experimental investigation of this kind. The children were thoroughly accustomed to the introduction of new methods of

¹ *Op. cit.*

² *Ibid.*

³ *Correlations of Mental Abilities*. New York, 1912.

⁴ The writer here desires to express his grateful thanks to Mr W. J. Deeley and Mr A. S. Harrison, successive Senior Masters of the Fielden Schools, for the generous manner in which they made possible the application of the tests; also to Dr P. Sandiford, the Superintendent of the Fielden School, for his permission to carry out the tests in the Schools.

112 *Quantitative Investigation of Mental Processes*

teaching and hence mental tests were not entirely foreign to their daily experience.

(b) A group of 41 children (Group II) at the Manchester High School for Girls¹. Their ages ranged from 10 to 12 years. Owing to the inconvenience caused by dislocation of the curriculum, the number of tests had here to be curtailed. The tests employed were carried out during school hours, and the children were led to believe that they formed an integral part of the school curriculum. The tests at this school were administered by the Headmistress of this section of the school, the method of procedure being similar to that adopted with the previous group.

In all these tests, the eagerness of the children was remarkable. Often, at the end of each performance, the cry for more was raised. The children entered into the work with far greater zeal and interest than was shown in the case of ordinary subjects of the curriculum. During the whole of the session this spirit was maintained; and when the tests were concluded, a general feeling of profound regret was expressed.

At the Fielden Demonstration Schools, a special room was set aside for the purposes of this investigation. It was situated in a quiet and undisturbed part of the building; in fact it was practically isolated from the rest of the school. The children were arranged around three sides of a rectangle, the experimenter being at the fourth. Each individual was thus under his direct supervision, and he was in full view of the class. Such an arrangement was found to be the most economical, especially with regard to the distribution and collection of papers. The experimenter could move rapidly round the inner side of the rectangle with a minimum of effort and a maximum of effect.

For demonstration purposes, a revolving blackboard was used. It could be distinctly seen by each child seated normally in his position in class, and was well suited to class experiments when a definite time of exposure was required. The stimulus word could be written on one side of the blackboard, and, by a quick rotation of the board, could be almost instantaneously exposed to the class.

During the tests the experimenter noted any peculiarities of the children as shown by their outward expressions. Throughout the whole session the proceedings were most automatic; the children

¹ To Miss Sarah A. Burstall, the Headmistress, for her kindness in allowing the use of the School, the writer tenders his best thanks; he is also greatly indebted to Miss Harrison of the Junior School, for her invaluable assistance in the application of the tests.

always awaited eagerly the signal to start; perfect stillness prevailed whilst the tests were in progress, and there was an immediate response to the command 'Stop.' Thus the conditions appeared to be as perfect as it was possible to make them, and the chance of obtaining accurate and reliable results was thereby increased.

At the Fielden Demonstration Schools, a classification of the children according to the order of their 'intelligence' was obtained from the master in charge of the class. This classification was based, not upon the results of any class examinations, but upon the master's opinion of the intellectual capacities of each of the children. Such a method of classification may be expected to result in coefficients of correlation which have a higher numerical value than those obtained from the High School for Girls, where the results of class examinations were used as data from which the correlations with intelligence were calculated.

IV.

In the case of every test, prolonged preliminary trials were made, and as a result some tests were discarded as impracticable whilst others were omitted as irrelevant to the aims of the investigation. Those were retained which showed indications of being representative of the higher intellectual capacities; a few memory tests were also included¹. Eventually the following tests were adopted, and were applied in the order given (for the sake of brevity the usually accepted names of these tests are given here, details are given later):

- | | |
|---------------------------|---|
| 1. 'E, R' test. | 10. Rearranged Letters. |
| 2. 'A, N, O, S' test | 11. Memory (immediate reproduction) for nonsense syllables. |
| 3. Word-building. | 12. Memory (delayed reproduction) for nonsense syllables. |
| 4. Sentence Construction. | 13. Memory (immediate reproduction) for letter squares. |
| 5. Analogies. | 14. Cross-Line Test. |
| 6. Completion. | 15. Interpretation of Fables. |
| 7. Missing Digits. | |
| 8. Part-wholes. | |
| 9. Dissected Pictures. | |

In the case of Group I the tests were repeated after an interval of six weeks. Only the first eight tests were applied to Group II.

1. '*E, R*' Test. A number of letters of the alphabet (600) were printed on papers, each paper containing twenty-one each of the letters E and R, and twenty-one each of the letters A, N, O, S for the next

¹ The tests carried out at each sitting never exceeded three in number, and in the case of the longer tests only two were given.

114 *Quantitative Investigation of Mental Processes*

test. The remaining letters were drawn as equally as possible from the rest of the alphabet. An example (using different letters) was first given on the blackboard, and then the papers were distributed face downwards. The children were told that they must cross through every E and R, beginning at the top of the page and proceeding downwards line by line. They were advised to work as quickly as possible but at the same time to avoid passing over any of the presented letters. At a given signal they turned the papers over and began to cross through the letters. At a second signal, two minutes later, they turned the papers face downwards again, and these were then collected.

System of marking and results: 1 mark for every letter crossed through correctly; - 1 mark for every letter omitted or crossed through incorrectly.

	Group I	Group II
Mean	17.75	12.3
Standard Deviation (σ)	6.4	12.8
Reliability Coefficient	.72	—
Correlation with Intelligence (r)	.40	.37
P. E. of r	.097	.098

2. '*A, N, O, S*' Test. The material and method of procedure were similar to the last test, but here the children were required to cross through the letters A, N, O, S.

The time allowed was three minutes; different children having been previously tested both in this and the 'E, R' test, in order to ascertain the time required to *complete* the test. The three minutes and two minutes were found to be just insufficient for the completion of the respective test. The letters A, N, O, S were written on the blackboard to act as a reminder in cases of forgetfulness.

System of marking and results: the system of marking was that adopted in the previous test.

	Group I	Group II
Mean	41.2	37.0
Standard Deviation (σ)	12.1	16.4
Reliability coefficient	.64	—
Correlation with Intelligence (r)	.45	.32
P. E. of r	.09	.099

3. *Word-building Test*. The children were provided with papers containing the letters A, E, O, B, M, T typed on the top, and they were required to construct, from the letters given, as many words as they could. The words were to contain any number of the letters from two to six (inclusive) but letters other than those given were not to be used.

Further, no letter could be used more than once in the same word, and the words allowed as correct must be found in a standard dictionary. A preliminary test was given with the letters E, A, I, R, L, P in order to acquaint the children with the nature of it, and to encourage questioning on doubtful points. The test papers were given out face downwards, and at a given signal the children turned them over and commenced to work. The time allowed was five minutes, at the end of which period a second signal was given and the papers were again turned over and collected.

System of marking and results: 1 mark was given for each word correct.

	Group I	Group II
Mean	10.5	12.5
Standard Deviation (σ)	4.08	4.42
Reliability Coefficient	.88	—
Correlation with Intelligence (r)	.58	.50
P. E. of r	.07	.08

For the second application of this test the letters were rearranged thus: O, A, E, M, T, B.

4. *Sentence Construction*¹. A list of ten words, expressing either concrete or abstract ideas, was given. The children were asked to construct sentences each containing a pair of successive words. They were told that the sentences must be constructed so as to show the closest possible connexion between the words used. Only the exact word given might be used; if singular, then the use of the plural was forbidden. A trial series of ten words was first given, so as to make the children familiar with the nature of the test.

The words of the test proper were printed in large, plain type on the unexposed side of the revolving blackboard. After warning the children to be on the alert, the board was quickly turned so as to expose the list of words, and the stop-watch was set going. On perceiving the words, the children immediately began to construct sentences containing them; writing down the results on papers. At the expiration of $2\frac{1}{2}$ minutes the children ceased work and the papers were collected. The list selected was such that each word was in some manner related to the succeeding word, *e.g.* Circle, Moon, Night, Sleep, etc.

System of marking and results: 5 marks were given for each sentence showing the closest possible connexion between the words; 4, 3, 2, 1 or 0 marks were given according to the degree of deviation from this standard.

¹ This is a modification of a test devised by Mr H. S. Lawson of Buxton College.

116 *Quantitative Investigation of Mental Processes*

The following are quoted from actual examples :

- (1) The full *moon* is like a *circle*. (5 marks.)
- (2) The *moon* is *circular* in shape. (4 marks.)
- (3) A ball is a *circle*, I thought it was the *moon*. (3 marks.)
- (4) The *circle moon* is round. (2 marks.)
- (5) A *circle* is round and the *moon* is bright. (1 mark.)
- (6) The *circle* and *moon*. (0 marks.)

	Group I	Group II
Mean	25.4	25.0
Standard Deviation (σ)	8.45	8.12
Reliability Coefficient	.83	—
Correlation with Intelligence (r)	.62	.60
P. E. of r	.07	.07

For the second test a different list of words was used, but presenting about the same amount of difficulty. In this test an irrelevant factor is the time taken in writing down the sentences; it varies much according to the length of the sentence to be written, and the subject's speed of writing. A sentence may be perfectly correct, but of greater length than another of equal value; the time taken in writing the one will be much greater than that of the other, a fact which will certainly tend to make the results misrepresentative.

A child who is obviously inferior to another in mental ability may, by constructing shorter sentences and by writing more quickly, equal or even surpass the latter in this test. The sentences of the one may be very simple but correct; those of the other may be longer but involve deeper thought.

5. *Analogies Test.* This test is based on the principle of proportional parts. Three terms were given, and the children were required to find a fourth. Thus in the following examples, which were used to illustrate the test, the children were presented with a definite relationship as expressed by the first and second terms; a third term was given, bearing the same relation to the unknown term as the first term bore to the second.

- (1) Storm : Calm :: War : x ?
- (2) Arm : Elbow :: Leg : x ?
- (3) Good : Better :: Much : x ?
- (4) Sound : Echo :: Seedtime : x ?
- (5) Known : Unknown :: Present : x ?

Much time was spent in making clear the nature of the test, the children themselves being asked to construct examples in order to show

how far the instructions had been understood. Twenty-five such examples were printed and the papers given out face downwards; at a given signal the children turned them over and began the test. On a second signal being given, five minutes later, the papers were again turned face downwards and immediately collected. The children were allowed to pass over any example which presented difficulties. Examples requiring general knowledge only were given.

System of marking and results: 4 marks were given for each correct solution; 3, 2, 1 or 0 marks were given for partially correct solutions according to the degree of correctness.

	Group I	Group II
Mean	45.7	54.7
Standard Deviation (σ)	22.8	16.1
Reliability Coefficient	.92	—
Correlation with Intelligence (r)	.80	.62
P. E. of r	.04	.07

The correlation with Intelligence is remarkably high (.80 in Group I) and individual differences were fairly well defined. In many cases inability to perceive relations was obvious, words being supplied which were distinctly irrelevant. In this test the examples may be constructed so as to present varying degrees of difficulty and consequently may be applied to children of widely different ages and ability.

6. *Completion Test.* This test consists in supplying the missing words in a passage of prose from which a number of words had been omitted. In order to acquaint the children with the requirements of the test, a preliminary trial was first given; this brought to light any cases of misunderstanding of the directions.

The papers were given out face downwards, and at a given signal the children turned the papers over and commenced to read through the piece carefully with a view to grasping its general meaning. Nothing was written during this period, which lasted for five minutes, but at its termination a second signal was given and the children then began to fill in the blanks. It was suggested that if a certain elision presented difficulty, it could be passed over for the time being, as the succeeding context would probably supply the necessary cue. This period lasted for ten minutes, at the end of which a signal was given for the children to stop.

The passage used was the following¹:

¹ Given in Whipple: *Manual of Mental and Physical Tests*. Baltimore, U.S.A., 1910, 448.

One.....eagle.....with the.....birds
see.....could.....highest.....agreed
he who.....fly.....should.....called.....
 strongest.....All started.....same.....and
away among.....clouds. One by.....they
 weary.....re....., but.....eagle.....upward and
until.....was.....mere speck.....heavens. When he
back,others werefor him; and.....
 touched.....a linnet.....off.....back where
hidden and.....that.....himself
strongest.....“.....stronger.....
;” said the....., “for not.....did I
as high, but.....he begandownward.....
, I.....my hidingandup.....little
this boastfulthe.....
 their heads and.....council tothe matter. After.....
 long.....decidedthe.....
the.....bird,not only.....he
so high, but.....the.....as well.
 To.....dayplumes.....
 are emblems of str.....and cou.....

System of marking and results: each blank filled in correctly was awarded 1 mark; each blank filled in incorrectly was awarded - 1 mark; each blank passed over was awarded - $\frac{1}{2}$ mark.

	Group I	Group II
Mean	34.6	42.0
Standard Deviation (σ)	13.8	11.6
Reliability Coefficient	.89	—
Correlation with Intelligence (r)	.85	.61
P. E. of r	.04	.07

For the purpose of repetition, the same passage was given and the method of procedure was the same except that the five-minute period for reading through the passage was omitted. The test thus occupied ten minutes, the children starting to fill in the blanks immediately. The correlation with Intelligence is unusually high (.85 in Group I) and hence this and the previous test would seem to offer very accurate indications of the child's level of intelligence.

This test correlates highly with the Analogies Test (.85); this being the highest correlation of any one of the tests with any other.

7. *Missing Digits*. This is a test which involves an entire modification of the habitual methods of working. An unfamiliar situation is presented to the subject, and the facility with which he can escape from his fixed habits in order to cope with this novel situation affords some indication of the degree of intelligence he possesses. The examples

given were illustrations of the four common arithmetical processes of Addition, Subtraction, Multiplication, and Division; they were of the most elementary type, and no special knowledge was required to effect their solution.

Example :	2•94
	•867
	781•
	<hr/>
	•42•6

Various digits in each example were omitted, the omissions being denoted by a dot. Papers, which contained one example each of the four classes named, were given out face downwards, and at a given signal the children turned them over and began to insert the missing digits. Five minutes later a second signal was given, whereupon the class again turned the papers over. It should be noted that no indication was given of the class to which each example belonged, except that a preliminary test, containing one example of each type, was first given.

System of marking and results: + 4 was given for each example wholly correct; - 4 was given for each example wholly incorrect and + 2, 0, or - 2 was given for each example partially correct according to the extent of error.

	Group I	Group II
Mean	2•85	6•4
Standard Deviation (σ)	5•76	6•65
Reliability Coefficient	•69	—
Correlation with Intelligence (r)	•65	•50
P. E. of r	•07	•08

The inserted digits were often incorrect, and as a result negative scores were obtained in many cases, thus making the average very low. Often the children failed to recognise the type of example given; frequent attempts being made to solve an example in Addition by means of the Multiplication process.

8. *Part-wholes.* In this test a list of ten words was given, and the children were required to write opposite each word the name of the whole of which the word given was a part. In order to familiarise them with the nature of the test, replies to the following examples were first elicited: eye, page, spire, wing, pillow.

The papers, on which the test words were printed, were given out face downwards, and at a given signal the children turned them over and began to write the appropriate words as quickly as possible. The

120 *Quantitative Investigation of Mental Processes*

time allowed was thirty seconds, at the end of which the papers were again turned face downwards, and then collected.

System of marking and results: 2 marks were given for each correct associate; 1 mark was given for each associate partly correct, and 0 marks were given for any incorrect associate.

	Group I	Group II
Mean	13.35	13.10
Standard Deviation (σ)	4.6	4.36
Reliability Coefficient	.65	—
Correlation with Intelligence (r)	.67	.56
P. E. of r	.07	.08

Unless the time allowed for this test had been just insufficient for anyone to complete it, the correlation with Intelligence would have been lower. The test is so easy that it could have been successfully accomplished by almost all the children if only sufficient time had been given, but the different rates at which the associates were evoked caused a differentiation closely related to the respective mental abilities of the children when the time allowed was only thirty seconds. Probably a longer list of words would make a better test.

9. *Reconstruction of Dissected Pictures.* This test was devised on the lines adopted by Burt¹. Each subject was provided with a picture postcard containing a reproduction of Gilbert's "Shylock, Salanio, and Salarino." This was divided into fourteen parts which were arranged in haphazard order, the same order, however, being maintained for each subject. This minimised the possibility of any chance selection of an easy or difficult fragment. The fragments were so arranged before the children entered the room, and were covered by papers. An intact postcard of the same scene was turned face downwards on the table. At a given signal this was turned over, and the children examined it carefully for thirty seconds. A second signal was given, on hearing which the children again turned the card face downwards, and then began to build up the picture from the disorganised fragments. The results were measured in terms of the time taken to reconstruct the pictures; a rather difficult procedure when only one experimenter was present. However, the following method, which enables measurements correct to two seconds to be taken, was devised.

The experimenter was provided with a stop-watch and a sheet of squared paper. The horizontal axis of the paper was marked off into

¹ *Op. cit.* 102.

divisions representing seconds, and the vertical axis into divisions representing minutes. Each subject was told to put up his hand on completing the task; the experimenter, being able to see each individual in the group, could note immediately such a movement, and at the same time could note the time on the stop-watch. If the experimenter throughout the test move his hand, holding a pencil, horizontally along the paper from left to right, at such a rate that the square on the paper at which the pencil is pointing is always equivalent to the time of the stop-watch, then on noting the raising of a hand, he can simultaneously place a *dot* on the paper, and the position of the dot represents the time correct to two seconds. The difficulty of knowing which subject corresponded to each dot was overcome by calling out a number as each hand was raised, according to the number of subjects who had already completed the test. Thus when the first to finish put up his hand, the experimenter called out 'ONE,' and the subject to whom it referred put this number down on his paper. The next to finish was given number 'TWO' and so on until all had completed the reconstruction. The experimenter could then read off at leisure the time taken by each subject, many of the times by this method being correct to one second. The method certainly involves alertness and coolness on the part of the experimenter; but when these are assured, it is quite successful.

	Group I
Mean	144 secs.
Standard Deviation (σ)	42.15
Reliability Coefficient	.92
Correlation with Intelligence (r)	.63
P. E. of r	.07

From the results with earlier trials with fewer fragments it would appear that the correlation with Intelligence is dependent upon the number and complexity of the cuttings. When the postcard is divided into eight rectangular pieces, the correlation with Intelligence is much lower. This is probably due to the fact that in such a case the times are too short for definite differentiation and the factor of fortuitous selection becomes relatively more pronounced. It is conceivable that such a test can be made to suit all grades of intelligence, from the most backward person to one of highest intellectual ability. The correlation with Intelligence is fairly high (.63), and a higher correlation might be expected by making the test more complex. The correlation with some of the other tests was not so high as one might have expected, this 'dissected pictures test' occurring rather low down in the 'hierarchy.'

122 *Quantitative Investigation of Mental Processes*

10. *Rearranged Letters.* In this test the letters of any number word (one, two, etc.) were arranged in haphazard order, and the children were required to find the particular number which could be spelt from the letters given. The words were printed on papers thus:

VFEI =

RFUO =

and opposite each word the children wrote the number down as a figure, thus:

VFEI = 5

RFUO = 4

Ten such words were used and the children were informed that only numbers from one to twenty (inclusive) would be given. Demonstrations on the blackboard were first given, and then the papers, each containing the ten words, were distributed face downwards. At a given signal the papers were turned over and the children began the test. One-and-a-half minutes later a second signal was given for the children to stop, and the papers were collected.

System of marking and results: 1 mark was awarded for each correct solution and - 1 mark for each incorrect solution.

	Group I
Mean	4.75
Standard Deviation (σ)	1.99
Reliability Coefficient	.65
Correlation with Intelligence (r)	.72
P. E. of r	.06

11. *Memory (immediate reproduction) for Nonsense Syllables.* The object of this test was to ascertain how far immediate mechanical memory correlates with Intelligence. For this purpose a list of ten nonsense syllables was printed in large, plain type on the revolving blackboard and was exposed to the view of the children for a period of three minutes. At the end of the three minutes the children counted from twenty backwards at the rate of one per second (the pace being set by the experimenter) and then began to write down the syllables remembered. By this means, attention was distracted from any primary visual memory image of the syllables that might otherwise have intruded.

No directions were given, as to how the syllables were to be learnt except that they must be reproduced as far as possible in the right order.

System of marking and results: 1 mark was awarded for each letter correctly reproduced; 1 mark was deducted for each error of transposition or of insertion of letters within syllables; a half-mark was deducted for each error of transposition of the syllables.

	Group I
Mean	14.0
Standard Deviation (σ)	8.7
Reliability Coefficient	.76
Correlation with Intelligence (r)	.59
P. E. of r	.07

Different lists of syllables were used in the two applications of this test. Among the results of the second test it was noticed that syllables were occasionally reproduced which had been given in the first test, generally in the correct position in the series, and yet the interval between the two tests was six weeks.

12. *Memory (delayed reproduction) for Nonsense Syllables.* In order to test retentiveness after an interval of exactly two days, the children were asked to write down the syllables they had previously learnt. No warning had been given that such a performance would be required of them, consequently it was unlikely that any effort had been made in the interval to retain the series.

System of marking and results: the system of marking adopted was the same as in the last test.

	Group I
Mean	5.25
Standard Deviation (σ)	7.1
Correlation with Intelligence (r)	.74
P. E. of r	.06

In connexion with the second series of nonsense syllables given, the amount retained after two days was not tested because some of the children, remembering the procedure in the earlier case, had endeavoured to retain the syllables learnt.

The difference between the means of this and of the previous test (5.25 and 14.0 respectively) indicates how rapid is the rate of forgetting in its earlier stages. In this test the correlation with Intelligence is very high, .74, while in the previous test it was .59; thus providing a measure of the degree to which intellectual capacities are more concerned in prolonged retention than in immediate recall. It is reasonable to assume that a longer interval between the learning of the series and their recall would give even a higher correlation with Intelligence.

124 *Quantitative Investigation of Mental Processes*

Some children scored more in this test than in the immediate memory test; more was actually remembered after an interval of two days than immediately after the exposure of the syllables, thus indicating that 'consolidation' had been at work in the meantime.

This is a test which correlates highly with those tests high in the 'hierarchy' (the correlations ranging from .5 to .7) and also with 'Immediate Memory for Nonsense Syllables' (.71). Thus those children who were best able to retain senseless material also did best in those tests which give the highest correlations with Intelligence.

13. *Memory (immediate reproduction) for Letter Squares.* The material for this test consisted of all the consonants of the alphabet; these were plain letters cut out of white paper and gummed on to stiff dark grey paper. The letters were $5\frac{1}{4}$ inches high and $3\frac{1}{2}$ inches wide, and the grey background of each letter was $7\frac{1}{4}$ inches by 7 inches,—a size sufficiently large for all to see conveniently. The letters being separate allowed of any possible combination being readily formed. For the purpose of exposure the revolving blackboard was used. It was found convenient to use this test as the first test of the sitting; the letters could be fastened on to the blackboard by means of drawing pins before the children entered the room, thus no waiting was necessary.

The nature of the test was first explained to the children but no directions were given as to how the letters should be learnt. The number of letters exposed at a time was twelve, and the children were provided with papers ruled into twelve squares for the purpose of reproducing in the right order, as far as possible, the letters learnt.

The children were warned to look at the blackboard; this was then rotated quickly so as to expose the letters, and at the same time the stop-watch was set going. At the end of twenty-five seconds the blackboard was again turned over and five seconds were allowed to pass before the children began to fill in the squares; thirty seconds being allowed for the actual reproduction.

The letters used for the first and second tests were:

1st test				2nd test			
M	T	D	X	K	X	P	R
V	L	P	N	C	F	L	Z
S	Z	Q	R	H	W	Q	U

System of marking and results: 3 marks were given for each letter in the correct position; 2 for each letter one remove to the right, left,

above, or below; 1 for each letter two removes to right, left, above, or below, *e.g.*

M	D	X	T	} 24 marks out of a possible 36.
(3)	(2)	(2)	(1)	
S	L	P	R	
(2)	(3)	(3)	(2)	
V	H	R	Q	}
(2)	(0)	(2)	(2)	

	Group I
Mean	17.4
Standard Deviation (σ)	4.06
Reliability Coefficient	.75
Correlation with Intelligence (r)	.18
P. E. of r	.11

This test gives the lowest correlation with Intelligence (.18) and is also lowest in the 'hierarchy.' It thus appears that intellectual processes function only slightly in this test, the process of imagery being relatively more prominent.

With some tests such as Word-building, Completion, and Missing Digits, it shows scarcely any correlation. The correlation with Intelligence is remarkably different from that of Memory for Nonsense Syllables and Intelligence (.59). Considering that both are 'memory tests,' the results seem to point to the fact that different mental processes are involved in the two methods. In addition to the important part played by imagery in the Letter-square test, the recall was more immediate than in the Nonsense Syllable test. Further, a relatively longer time was allowed for the learning of the syllables, and from introspections obtained, the memory of the backward children suffered more at the hands of the 20 seconds period than did that of the intelligent. The learning of the nonsense syllables requires more effort and concentration of attention than the learning of the letters, and as the duller children have not the same power of keeping their attention focused on a task over a considerable period, this factor may have been instrumental in causing the widely different results.

14. *Cross-Line Test*¹. As a preliminary test the following figure was drawn on the blackboard:



¹ "Tests for Practical Mental Classification," by Wm. Healy and Grace M. Fernald. *Psychol. Rev. Psychol. Monogr.* 1911, XIII.

126 *Quantitative Investigation of Mental Processes*

It was pointed out to the children that the figure was made up of four compartments, each containing a number. These compartments were then drawn separately thus:



and the children were asked to give the number to which each compartment corresponded. When it was apparent that the children understood the requirements of the test, the figure proper (Fig. 1),

1	2	3
4	5	6
7	8	9

Fig 1.

having been previously drawn on the reverse side of the black-board, was exposed for a period of twenty seconds. During this period the children examined carefully its constitution. Papers, on which the separate compartments (Fig. 2) were printed, had been previously



Fig. 2.

given out face downwards. The children now turned these over and commenced to write in each compartment the number corresponding to it. When one subject had completed the test, the signal to stop was given, and the papers were collected.

System of marking and results: 1 mark was given for each number correct; - 1 mark for each number incorrect.

	Group I
Mean	3.7
Standard Deviation (σ)	4.44
Reliability Coefficient	.82
Correlation with Intelligence (r)	.46
P. E. of r	.09

For the second application of the test the numbers in the compartments were redistributed¹.

¹ This of course involves the difficulty that a previously formed association has to be dissolved before the new one is formed.

The correlation with Intelligence (.46) is lower than usual; this is explicable upon the supposition that the successful interpretation of this test is closely related to vividness of visual imagery. The correlations with the other tests are low, generally lying between .2 and .4 except in the case of the Dissected Pictures test, the correlation here rising to .54. Since both these tests involve memory of spatial relations and the recognition of parts, their comparatively high correlation is quite conceivable. The Letter-Square test also gives a higher correlation with the Cross-Line test than with any other; but both these tests involve imagery, and probably visual imagery in particular.

15. *Interpretation of Fables.* This test was introduced in the hope that it would give some indication of the child's ability to grasp the meaning of a passage of prose in the form of a fable. The whole point or moral of a fable can usually be expressed in a very few words, whilst the inherent interest makes it easy to hold the whole story in the mind when endeavouring to extract the meaning.

The following five fables were used¹:

- (1) The Boy and the Filberts.
- (2) The Stork and the Cranes.
- (3) Mercury and the Woodman.
- (4) The Eagle and the Tortoise.
- (5) The Ants and the Grasshopper.

Each fable was read twice by the experimenter, the second reading following immediately after the first. At the end of the second reading the children wrote down what they thought to be the moral of the fable. This process was repeated with each of the remaining fables.

System of marking and results: 5 marks were given for each correct interpretation; 4, 3, 2, 1, or 0 marks were given for incorrect interpretations, according to the degree of deviation from the correct reply.

	Group I
Mean	11.5
Standard Deviation (σ)	5.7
Correlation with Intelligence (r)	.64
P. E. of r	.07

This test was not repeated, as afterwards some of the children pondered over the fables given, and hence made the conditions for

¹ These fables are part of a selection originally made by L. M. Terman, and are to be found in "A Tentative Revision and Extension of the Binet-Simon Measuring Scale of Intelligence," by L. M. Terman and H. G. Childs. *J. of educ. Psychol.* 1912, III. No. 3.

128 *Quantitative Investigation of Mental Processes*

a repetition unequal. The interpretations were very varied; some showing no connexion whatever with the original passage; others exhibiting a thorough grasp of the matter and displaying ability in formulating the moral. None of the children had previously made the acquaintance of any of the fables.

V.

Throughout this investigation the product moment formula $\frac{S(xy)}{N\sigma_1\sigma_2}$ was employed. The coefficients of correlation were calculated from the results obtained by amalgamating the two measurements made in each test, and the reliability coefficients represent the extent of the correlation between the series obtained by adding the first half of the first performance to the second half of the last performance, and by adding the second half of the first performance to the first half of the last performance.

In Tables III and IV (see pages 131, 132) the coefficients of correlation of each test with every other are given. These are arranged in such a manner that the sums of the columns or rows of coefficients are in descending order of magnitude from left to right or from above downwards.

Comparison of Groups I and II. The following table gives the mean, standard deviation (σ), and correlation with Intelligence (r) of each of the tests applied to the two groups.

TABLE I.

Test	Group I			Group II		
	Mean	σ	r	Mean	σ	r
E, R	17.75	6.4	.40	12.3	12.8	.37
A, N, O, S	41.2	12.1	.45	37.0	16.4	.32
Word-building	10.5	4.08	.58	12.5	4.42	.50
Sentence construction	25.4	8.45	.62	25.0	8.12	.60
Analogies	45.7	22.8	.80	54.7	16.1	.62
Completion	34.6	13.8	.85	42.0	11.6	.61
Missing Digits	2.85	5.76	.65	6.4	6.65	.50
Part-wholes	13.35	4.6	.67	13.10	4.36	.56
Mean	23.9	9.75	.63	25.4	10.05	.51

In those tests which give the highest correlations with Intelligence the means of Group II are higher than those of Group I, although the

average age of the former group is slightly less than that of the latter. The children of Group II were drawn from the wealthier parents, and consequently it appears justifiable to infer that in this case, at least, the children of superior intelligence belong to the socially superior parents. This view is in harmony with Burt's conclusions¹.

The rank correlation² of r for Groups I and II is .89; which indicates that the relative value of the tests as a means of indicating 'general intelligence' is approximately the same in both groups.

The correlations with Intelligence are unusually high in the case of Group I. This is probably due to the following factors:

- (1) The teacher's estimate of intelligence was not based on the class lists, but on his own opinion of the respective intellectual capacities of the children;
- (2) the perfect conditions which prevailed during the application of the tests;
- (3) prolonged preliminary trials;
- (4) absence of irrelevant factors.

All the tests in Group I were applied by the writer, and thus any discrepancy due to the presence of an untrained experimenter was eliminated. Also it is evident from the table that in some cases the standard deviations of the same tests are widely different in the two groups.

Comparison of Results with those of other investigators. In the following table the results of this investigation are given along with those of other investigators who have employed similar tests. The values represent the coefficients of correlation between various tests and the subjective estimations of intelligence.

The upper and lower lines of Brown's results represent his Groups II and III respectively. Similar representation of the writer's results indicates respectively Groups I and II.

Thus the coefficients obtained by the writer are generally higher than those obtained by the other investigators quoted; the difference in all probability being due to the reasons already given (see p. 109 above).

Evidence of a General Common Factor. According to Hart and Spearman³, if a General Factor underlies all the performances tested,

¹ "Experimental Tests of General Intelligence." *This Journal*, 1909, III. 176.

² An explanation of the method of determining the rank correlation is given by Brown, *op. cit.* p. 50; also in an article by P. Sandiford: "Educational Measurements," *J. of exp. Ped.* 1912, I. 217.

³ *Op. cit.* p. 59.

TABLE II.

Test	Burt ¹	Brown ²	Wyatt
Completion48	.43	.85
Analogies52	.69	.61
Erasure of letters E, R39	—	.80
Erasure of letters A, N, O, S	—	0	.62
Mechanical memory.....	.43 ³	.28	.40
Sentence construction75	.13	.37
Dissected pictures.....	.62	.10	.45
	[.72]	.55	.32
		.49	.59
		—	—
		—	.62
		—	.60
		—	.63
		—	—

the correlation between the pairs of columns of coefficients in Table III should be high and positive; that is, the correlation must approach unity in proportion as the coefficients admit of perfect hierarchical arrangement. To correct for the bias due to the presence of sampling errors they give the following formula⁴

$$R'_{ab} = \frac{S(\rho_{xa}\rho_{xb}) - (n-1)r_{ab}\sigma_{xa}\sigma_{xb}}{\sqrt{S(\rho^2_{xa}) - (n-1)\sigma^2_{xa}}\sqrt{S(\rho^2_{xb}) - (n-1)\sigma^2_{xb}}},$$

where R'_{ab} is the correct value to be obtained

$S(\rho^2_{xa})$ is the sum of the squares of the differences of the coefficients of column a from their mean, and similarly $S(\rho^2_{xb})$,

σ_{xa} is the probable error of r_{xa} divided by .6745 and similarly σ_{xb} .

To exclude columns which are influenced by the presence of large sampling errors, Hart and Spearman require⁶ "that in each of these columns the mean square deviation should be at least double the correction to be applied to that deviation." In the present investigation only five pairs of columns reach this prescribed standard⁷; these are

¹ *J. of exp. Ped.*, *op. cit.* p. 111.

² *Op. cit.* pp. 115, 116.

³ *This Journal*, *op. cit.* p. 145.

⁴ *Ibid.* p. 82.

⁵ $S(\rho_{xa}\rho_{xb})$ is obtained in the form $\frac{S(\rho^2_{xa}) + S(\rho^2_{xb}) - S(\rho_{xa} - \rho_{xb})^2}{2}$,

this equals $\frac{S(\rho^2_{xa}) + S(\rho^2_{xb}) - [S(\rho^2_{xa}) - 2S(\rho_{xa}\rho_{xb}) + S(\rho^2_{xb})]}{2} = S(\rho_{xa}\rho_{xb})$.

⁶ *Op. cit.* p. 56.

⁷ The writer here desires to acknowledge his indebtedness to Prof. Spearman, who kindly supplied some corrections to the method adopted in determining the correlation between the columns of coefficients.

Analogies and Word-building	=	·93
Completion and Word-building	=	·97
Completion and Part-wholes	=	1·05
Word-building and Part-wholes	=	·99
Part-wholes and Memory (delayed)	=	·92
Mean correlation between columns	=	·97

TABLE III. (GROUP I.)

Test	Analogies	Completion	Word-building	Part-wholes	Rearranged letters	Memory (delayed)	Missing digits	Sentence Construction	Fables	Nonsense syllables	Dissected pictures	E, R	A, N, O, S	Cross-line	Letter-squares
Analogies	—	·85	·65	·67	·63	·70	·61	·63	·74	·64	·50	·43	·54	·40	·28
	—	·04	·07	·07	·07	·06	·07	·07	·06	·07	·08	·09	·08	·10	·10
Completion	·85	—	·70	·75	·62	·72	·60	·71	·69	·61	·41	·47	·43	·33	·03
	·04	—	·06	·06	·07	·06	·07	·06	·07	·06	·10	·09	·09	·10	·12
Word-building	·65	·70	—	·77	·63	·66	·72	·52	·47	·57	·30	·56	·49	·15	·006
	·07	·06	—	·05	·07	·07	·06	·08	·09	·07	·10	·08	·09	·11	·12
Part-wholes	·67	·75	·77	—	·59	·51	·57	·67	·56	·52	·36	·43	·46	·24	·09
	·07	·06	·05	—	·07	·08	·07	·07	·08	·08	·10	·09	·09	·10	·11
Rearranged letters ..	·63	·62	·63	·59	—	·61	·63	·50	·38	·48	·53	·65	·41	·32	·16
	·07	·07	·07	·07	—	·07	·07	·08	·10	·09	·08	·07	·10	·10	·11
Memory (delayed).....	·70	·72	·66	·51	·61	—	·63	·34	·43	·71	·53	·34	·40	·33	·15
	·06	·06	·07	·08	·07	—	·07	·10	·09	·06	·08	·10	·10	·10	·11
Missing digits	·61	·60	·72	·57	·63	·63	—	·60	·41	·38	·54	·40	·46	·42	·03
	·07	·07	·06	·07	·07	·07	—	·07	·10	·10	·08	·10	·09	·10	·12
Sentence Construction	·63	·71	·52	·67	·50	·34	·60	—	·53	·30	·36	·30	·45	·28	·14
	·07	·06	·08	·07	·08	·10	·07	—	·08	·10	·10	·10	·09	·10	·11
Fables	·74	·69	·47	·56	·38	·43	·41	·53	—	·41	·26	·20	·37	·23	·31
	·06	·06	·09	·08	·10	·09	·10	·08	—	·10	·10	·11	·10	·11	·10
Nonsense syllables ...	·64	·61	·57	·52	·48	·71	·38	·30	·41	—	·26	·29	·06	·29	·25
	·07	·07	·07	·08	·09	·06	·10	·10	—	·10	·10	·10	·12	·10	·10
Dissected pictures.....	·50	·41	·30	·36	·53	·53	·54	·36	·26	·26	—	·28	·26	·54	·18
	·08	·10	·10	·10	·08	·08	·08	·10	·10	·10	—	·10	·10	·08	·11
E, R	·43	·47	·56	·43	·65	·34	·40	·30	·20	·29	·28	—	·57	·17	·11
	·09	·09	·08	·09	·07	·10	·10	·10	·11	·10	·10	—	·07	·11	·11
A, N, O, S	·54	·43	·49	·46	·41	·40	·46	·45	·37	·06	·26	·57	—	·20	·07
	·08	·09	·09	·09	·10	·10	·09	·09	·10	·12	·10	·07	—	·11	·12
Cross-line	·40	·33	·15	·24	·32	·33	·42	·28	·23	·29	·54	·17	·20	—	·32
	·10	·10	·11	·10	·10	·10	·10	·10	·11	·10	·08	·11	·11	—	·10
Letter-squares	·28	·03	·006	·09	·16	·15	·03	·14	·31	·25	·18	·11	·07	·32	—
	·10	·12	·12	·11	·11	·11	·12	·11	·10	·10	·11	·11	·12	·10	—

132 *Quantitative Investigation of Mental Processes*

Thus the correlation between the pairs of columns is high and positive, and the contention of Hart and Spearman that a General Common Factor exists receives further support.

TABLE IV. (GROUP II.)

Test	Completion	Part-wholes	Analogies	E, R	Word-building	Missing digits	A, N, O, S	Sentence Construction
Completion	— —	.69 .06	.58 .07	.58 .07	.36 .10	.45 .08	.54 .08	.57 .07
Part-wholes69 .06	— —	.54 .08	.48 .08	.36 .10	.38 .09	.32 .10	.53 .08
Analogies58 .07	.54 .08	— —	.19 .11	.54 .08	.54 .08	.14 .11	.39 .09
E, R58 .07	.48 .08	.19 .11	— —	.39 .09	.31 .10	.57 .07	.27 .10
Word-building36 .10	.36 .10	.54 .08	.39 .09	— —	.51 .08	.29 .10	.39 .09
Missing digits48 .08	.38 .09	.54 .08	.31 .10	.51 .08	— —	.37 .09	.00 —
A, N, O, S54 .08	.32 .10	.14 .11	.57 .07	.29 .10	.37 .09	— —	.29 .10
Sentence Construction	.57 .07	.53 .08	.39 .09	.27 .10	.39 .09	.00 —	.29 .10	— —

VI.

The following conclusions may be drawn from this investigation :

1. The Analogies and Completion Tests give the highest correlations with the subjective estimations of intelligence¹. The inter-correlations of each of these tests with every other are also high.

¹ The practical value of these tests as a means of classifying children according to the teachers' estimates of intelligence has since been demonstrated at the Fielden School, Manchester. The tests were applied to seven children who desired to be admitted to the school, and the papers were sent to the writer to be marked. From the results the writer was able to suggest that five of the children should be placed in certain classes and that the other two should not be admitted to the schools. The Senior Master, who had interviewed each prospective scholar, and also tested their knowledge of arithmetic, reading, and writing, entirely agreed with the writer's classification, and accordingly the

2. The correlational coefficients of each test with every other admit of hierarchical arrangement; the theory of the General Common Factor thus receives further support.

3. Memory of the kind displayed in the retention of nonsense syllables, seems to be a prominent factor in the mental processes of the higher levels. The correlational coefficients between memory for nonsense syllables and those tests which correlate highly with the subjective estimation of intelligence range from .5 to .7.

4. The children who are lowest in the scale of intelligence (according to the teacher's estimate) are least able to retain senseless material over a period of two days.

5. Some of the tests which are closely related in content give relatively low coefficients of correlation with each other.

6. The rank correlation between the coefficients of correlation (with Intelligence) of Groups I and II amounts to .89. This signifies that the order of the performances measured tends very largely to be the same in both groups, and consequently the relative values of the tests as a means of estimating intelligence shows a very high degree of similarity in the two groups.

7. The Letter-Square Test gives a higher correlation with the Cross-Line Test than with any other. As both these tests involve imagery, and probably visual imagery in particular, the comparatively high correlation between them may be due to this fact. The correlation of the Letter-Square Test with the Intelligence Classification is very low.

8. In those tests which give the highest correlations with intelligence, the performances of the children of Group II are superior to those of Group I.

9. In a random group of thirty subjects, though there are well-marked individual differences, yet there are no abrupt transitions between them.

children were dealt with as suggested. The class-work of each of the children admitted has since confirmed the accuracy of the classification. The writer was equally successful in predicting each subject's approximate position in class at the end of the term. Other results which will further demonstrate the utility of these tests as a means of indicating the child's level of intelligence, will be published later.

(Manuscript received 13 March, 1913.)

PUBLICATIONS RECENTLY RECEIVED.

Experimental Studies of Mental Defectives: a critique of the Binet-Simon Tests and a Contribution to the Psychology of Epilepsy. By Dr J. E. WALLACE WALLIN. Pp. vi + 155. Baltimore: Warwick & York, 1912. \$1.25.

This is one of the series of Educational Psychology Monographs, issued under the editorship of Prof. G. M. Whipple. The scope of the work, which appears to have been carried out with the necessary care and ability, is sufficiently indicated in the sub-title. The data were obtained during an eight months' residence of the author in the New Jersey State Village for Epileptics. Comparisons are attempted between the intelligence of the 333 epileptics examined by him in the Village and 378 feeble-minded inmates tested in Vineland, New Jersey, by Goddard. Unfortunately, as the author points out, certain differences in the methods of testing make such comparison difficult. Indeed the chief value of this careful piece of work lies not so much in the study of mental defectives as in demonstrating the need for improvement in the nature and arrangement of the Binet-Simon tests. By a comparison of his own results with those of other workers, he shows that some of the tests are too hard, while others are too easy, for children of a given age. He also gives reasons for preferring the 1908-form of the tests to the modified form proposed by Binet and Simon in 1911. The latter form, for example, omits the reading test, which the author shows to be a valuable test for differentiating the mental capacity of epileptics. But despite its present drawbacks, the author insists that the Binet-Simon scale "*does enable us to grade and classify defective individuals far more rapidly and satisfactorily than would be possible by the ordinary methods of observation*" (p. 104). He also insists that it is the function of this scale "to give us a *preliminary*, and not a *final* survey or rating of the individual who may be tested." It is to be regarded "*merely as a point of departure for further diagnosis*" (p. 109). In America, and doubtless in this country too, "the idea, unfortunately, seems to be gaining ground that anyone, be he grade teacher, introspective psychologist, practising lawyer or general medical practitioner, is able to make psychological diagnoses by putting the child through a few stock psychological tests. Nothing is more preposterous" (p. 110). When the tests end, the work of the expert psychiatrist begins.

A Psychological Study of Religion: its Origin, Function and Future. By Prof. J. H. LEUBA. Pp. xiv + 371. New York: Macmillan Co., 1912.

The author's main position in this interesting book is that while Psychology can say nothing as to the ultimate value of the metaphysical doctrines used as a setting for and as an intellectual interpretation of religious experience, the religious experience itself belongs wholly to Psychology, and is to be subjected to psychological analysis equally with the non-religious parts of conscious life. He denies that the religious consciousness is unique in the sense that it has specific affective experiences or psychologically distinctive desires and purposes; for him it is fundamentally the same as the non-religious consciousness. "Any impulse, any desire may lead to religious activity, and in it no type of emotion is to be found which is not represented also outside." Leuba contends that as the actual gods of historical religion are empirical gods, they are legitimately and entirely objects of science. Theology, on the other hand, makes its appeal to our inner experiences, either using them as the material for a quasi-scientific inductive proof of the existence of a divine power, or treating them as if they bore in themselves the marks of transhuman origin, as being immediate revelations of God. Leuba lays stress on the easy confusion between the factual existence and quality of a given subjective experience, on the one hand, and its transsubjective meaning and objective validity, on the other.

Psychotherapy. By Prof. H. MÜNSTERBERG. Pp. x + 401. London : Fisher Unwin, 1909. 8s. 6d. net.

In this work, Münsterberg discusses the psychological basis, the practice, and the place of psychotherapy—that kind of treatment which, in contrast to physiotherapy, seeks to treat the sick by influencing their *mental* life. The aim of the book is “to counteract the misunderstandings which overflow the whole field” and “to strengthen the feeling that the time has come when every physician should systematically study psychology, the normal in the college years and the abnormal in the medical school.”

The Game of Mind: a Study in Psychological Disillusionment. By P. A. CAMPBELL. Pp. 80. New York: The Knickerbocker Press, 1913. 75 c. net.

A bright but shallow philosophical jest, the purport of which may be gathered from the concluding sentences of the book. “With a sort of satisfaction we should put ourselves in the introspective frame of mind of admitting that our ever-changing mentality has its full source and flow in the organic life of the body. Of admitting that perceiving, conceiving, knowing, etc., are after all only finer sorts of bodily living. That remembering means reconstitution. That feeling is a discussion. That consciousness is self-analysis. That mental evolution, for its part, is a selective and mutatory handing on from the past, as a mechanistic gift to the present and future, of the great bodily GAME OF MIND.”

Insomnia: its Causes and Treatment. By Sir JAMES SAWYER. Pp. 107. Birmingham: Cornish Bros., 1912.

Eye-Strain in Everyday Practice. By S. STEPHENSON. Pp. viii + 139. London: The Ophthalmoscope Press, 1913. 3s. 6d. net.

In the Abstract. By NORMAN ALLISTON. Pp. 126. London: Swan Sonnenschein & Co., 1909. 2s. 6d.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL
SOCIETY.

- March 8, 1913. The Psychological System of Sigm. Freud, as set forth in Chap.
VII. of the "Traumdeutung," by W. BROWN.
Stern's *Tonvariator* (Demonstration), by W. BROWN.
The Analysis of some personal Dreams, with special reference to
Freud's Interpretation, by T. H. PEAR.
- May 3, 1913. Notes on a case of Morphomania, by F. AVELING.
Wonder, Fascination and Curiosity, by CARVETH READ.
-

ERRATA.

- Vol. v. p. 421, line 17, for " $\sqrt{\bar{r}_{ab}}$ " read " \bar{r}_{ab} ."
" " p. 421, line 29, for " $\sqrt{\bar{r}_{ab}}$ " read " \bar{r}_{ab} ."

ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE? I.

BY CHARLES S. MYERS.

I. Introduction.

- § 1. *Initial assumptions.*
- § 2. *'Intensiveness' 'extensiveness' and 'protensiveness.'*
- § 3. *Intensity and movement.*
- § 4. *Mental 'processes' and mental 'products.'*

II. The Nature of Intensity Changes.

- § 1. *The biological conditions of consciousness.*
- § 2. *The 'all or none' principle in spinal reflexes.*
- § 3. *The same principle in muscle-fibre.*
- § 4. *The same principle in nerve-fibre.*
- § 5. *The same principle in the heat and cold spot system of sensibility.*
- § 6. *Pain in relation to other forms of cutaneous sensibility.*
- § 7. *The grading of 'clonic' spinal reflexes.*
- § 8. *The grading of auditory sensations.*
- § 9. *The grading of 'tonic' spinal reflexes.*
- § 10. *The grading of 'tonic' sensations.*
- § 11. *Intensity, quality, and extensity in graded 'tonic' sensibility.*
- § 12. *Suggested relation of the attributes of colourless and colour sensations.*

III. Conclusions.

I.

§ 1. I ASSUME at the outset that the three following propositions will meet with general acceptance². The first is that, whereas Weber's law is a direct expression of the data of sense experience, Fechner's

¹ A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the Mind Association in London, 7 June, 1913.

² Reasons for accepting them will be found in my *Textbook of Experimental Psychology*, 2nd ed. 1911, I. 249-253.

law has no such immediately psychological basis, being derived from the application of mathematical symbols and processes to those data. The second is that we are incapable of *measuring* the intensities of sensations, *quâ* sensations; we cannot say that the sensation evoked by an arc light contains so many units of the sensation evoked by a candle light,—we can only range sensual intensities in a graded series. The third proposition is that *intensity* experiences and *intensity difference* experiences are fundamentally similar in their dependence on past experience and (unconscious or conscious) comparison, and in the conditions governing their respective (absolute and differential) thresholds. What may follow from the discussion on intensities in this paper will therefore apply also to intensity differences which are the subject of this symposium.

§ 2. From the acceptance, however, of these three propositions, it by no means follows that differences in sensual intensity (or that sensual intensities themselves) are qualitative and not quantitative. It may well be that sensual intensities are dependent for their increase or decrease on an increase or decrease in the quantity of sensation, notwithstanding our inability to isolate the component units of which any intensity may really be the sum.

Even in the case of a given experience of *extensity* of sensation, we can only say that one sensation is more or is less extensive than another. We can hence only arrange sensations in the order of their extensity; *quâ* sensation we cannot say that one is twice as extensive as another. But we *can* conclude that one line or area (not the *sensation* of a line or area) is twice as extensive as another; and here lies the difference between extensity and intensity. Whereas, on the basis of extensity, we can directly measure the 'extensiveness' of objects¹, we can only measure their 'intensiveness' indirectly in terms of 'extensiveness'—*e.g.* we express the intensity of visual or auditory stimuli in terms of units of amplitude (extensiveness) of wave movement, and the intensity of a weight or taste stimulus in terms of units of matter (extensiveness) of the lifted or tasted object.

Now just as sensual extensity, when conjoined with movement, assumes the form of quantitative extensiveness (spatiality), so sensual protensity, though immeasurable *quâ* protensity, is capable of giving rise to a quantitatively measureable 'protensiveness' (time). Indeed

¹ I agree with Ward: "...before and apart from movement altogether, we experience that massiveness or extensity of impressions in which movements enable us to find positions, and also to measure" (*Encycl. Brit.* Article).

extensity and protensity differ strikingly from intensity and quality, in that the experiences of space and time, to which the former give rise, are lived *through*, whereas those corresponding to intensity and quality are lived *in*; the one pair of experiences are essentially transitional, the other pair punctual. We can at the outset integrate extensity and protensity with movement in a way impossible in the case of intensity and quality.

§ 3. But nevertheless the connexion of intensity with movement is obviously intimate. Power and intensity are practically synonymous. The very word intensity means a state of tension or strain. What, then, is more natural than to suppose that our experience of intensity depends ultimately on the muscular strains exerted to resist force—or even, with Münsterberg¹, to deduce intensity experiences from kinaesthetic sensations?

But if differences in intensity, say of visual sensation, are to be traced to differences in intensity of kinaesthetic sensation, it is difficult to see why we should be able to appreciate, as we can, a difference between 100 and 101 units of intensity of two lights, whereas we are hardly able to appreciate the difference between 100 and 103 units of mass in two lifted weights². Moreover, the explanation is only pushed a step further back; we have yet to consider how sensations from the locomotor apparatus can vary in intensity.

§ 4. It is true that the sensations of muscular strain have in the past been held wholly responsible for the 'sense' (or 'feeling') of effort in all self-activity. Unquestionably afferent impulses of motor origin are of prime importance for our experience of activity, just as they and other (*e.g.* visceral) impulses are of prime importance for our experience of emotion. But there is now, I think, a growing consensus of opinion

¹ *Beitr. z. exp. Psychol.* 1890, Hft. 3, 23.

² I have perhaps unconsciously adopted this argument from A. Aliotta's *La misura in psicologia sperimentale* (Firenze, 1905, second footnote, page 106), which since writing the above I have re-read after a long interval. I may add that Aliotta criticizes Bergson's contention in *Les données immédiates de la conscience* (Paris, 1889, 32 ff.) that to determine values of a stimulus correspond particular qualities of sensation, and that from experience we are led to associate with each such *quality* of the effect (sensation) the idea of an intensive *quantity* of the cause (stimulus). But, asks Aliotta (pp. 105, 106), how do we know that the intensity of the stimulus is changing except by previous sensations of different intensity? How can we transfer the magnitude of *physical* objects to our *mental* experience unless the idea of intensity has already arisen in consciousness? Cf. also Knight Dunlap (*Psychol. Rev.* 1912, vi. 425, 426) for his criticism of Titchener's view (*Textbook of Psychology*, Pt. 2, 140) of the connexion between kinaesthesia and the perception of relation.

that the acts of the self are, in themselves, conscious, apart from sensations of peripheral origin, and that we should make two main divisions of consciousness—the consciousness of ‘acts’ or ‘processes’ (e.g. the ‘acts’ of attending, imagining, remembering, thinking, willing) and the consciousness of ‘contents’ or ‘products’ (e.g. ‘what’ we attend to, ‘what’ we imagine, ‘what’ we remember, ‘what’ we think, ‘what’ we will).

II.

§ 1. Consciousness makes its appearance in life whenever a choice on the part of the organism is possible between two or more reactions to a given stimulus. So long as behaviour is fixed (in living matter it is of course never *absolutely* fixed), there is no consciousness—or at all events no consciousness in which the organism’s ‘self’ shares. But with the development of instincts, fixity gives places to plasticity; a certain choice of reaction is open; a certain improbability is possible by practice and imitation; whereupon (as I have before insisted¹) a rudiment of intelligence at once comes into play.

Now the correlate of differences in *quality* of a sensation consists in differences in *type* of reaction. A sweet taste corresponds with one type of reaction, a bitter taste with another; similarly with the sensations of colour and pitch, different types of reaction are evoked from longer or shorter waves. In their most primitive form, reactions are mainly those of approach and withdrawal. Certain stimuli cause positive, others cause negative ‘taxis.’ So long as the taxis is fixed, sensation is useless. But with the evolution of the nervous system, as soon as plasticity replaces fixity, vague ‘affections’ of pleasure and pain enter, followed at a later epoch by the evolution of ‘sensations,’ the number of possible reactions to the same class of stimulus being simultaneously increased. At bottom, differences in type of movement must be the cause of differentiation in the quality of sensation; it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable in promoting different types of response².

¹ This *Journal*, 1909–10, III. 209–218, 267–270.

² Of course, in the developing adult we have to distinguish between the *inherited* physiological basis of sensual qualities (and intensities) and his successful differentiation of those qualities (and intensities) which is improvable by *practice*. That is to say, we have to distinguish between the primary influence of heredity and the secondary influence of education (environment), though the latter is ultimately dependent for its effects on heredity.

§ 2. If the qualities of sensation correspond to different types of reaction, we may be inclined to attribute the intensities of sensation to different degrees—moreness or lessness—of the same reaction. This, however, leads to the consideration of what is involved in moreness or lessness of a reaction. There are some reflex actions of the cord which apparently cannot be graded in strength. For example, by pressing or stretching the skin between or beneath the pads and cushion of the dog's hind foot, a reflex known as the 'extensor thrust' reflex is evoked; the leg is reflexly extended. (Reflexes can be best studied in the 'spinal' animal, in which the connexion between the cord and the higher parts of the central nervous system has been severed.) Now the extensor thrust reflex of the spinal preparation is little, if at all, altered by the strength of the external stimulus. So long as the external stimulus is adequate, whether it be relatively weak or strong, it produces practically the same strength of response¹. It is an instance of 'all or none' in reaction. The reflex thrust is of very short duration, being easily fatigable.

Hence, in the case of the extensor thrust reflex, a stimulus of suitable strength and of suitable character, applied in a given situation, evokes in the nervous tissue within the cord a certain pattern of response, the issue of which is the discharge of efferent impulses along certain nerve fibres supplying certain muscles; and within fairly wide limits, the movement resulting from this efferent discharge is independent of the strength of the ingoing stimulus.

Now if in the spinal animal these limits are grossly overstepped, we do not get an increased extensor thrust reaction; quite another type of reaction appears,—an immediate and well-marked flexion-reflex. In other words the afferent impulses, on reaching the cord, evoke quite another 'mechanism.' They evoke quite another pattern of nervous response within the cord, so that we get a very different discharge, causing the contraction, not of extensor, but of flexor muscles.

As we have seen, the extensor thrust reflex is an instance of an 'all or none' reaction. The reaction is either obtainable or unobtainable: the stimulus is either effective or not; there is no grading of the reaction comparable to the grading of the stimulus. Nor is this 'all or none' effect manifested in a reflex only. There can be little doubt that muscle fibres and peripheral nerve fibres follow the same principle.

§ 3. In the case of the *cutaneus dorsi* muscle of the frog (which is particularly suitable for investigation owing to its peculiar nerve

¹ C. S. Sherrington, *The Integrative Action of the Nervous System*, London, 1906, 74.

supply), Lucas¹ has been able to prove that the force with which the muscle contracts as a whole is due simply to the number of the individual muscle fibres involved in the contraction.

§ 4. Next, as regards nerve fibres (which are of more immediate interest)—thanks mainly to the work of Gotch² and Adrian³—there can be little doubt that the principle holds good for them as it does for muscle fibres. Gotch found (i) that the rate of propagation of the wave of excitation (as shown by the concomitant electrical changes within the nerve trunk) is the same whether the excitation is strong or weaker; and (ii) that the effects obtained by exciting only a portion of the nerve fibres of a nerve trunk closely resemble those evoked by a weaker stimulus applied to the entire nerve trunk. These results suggest that a stronger stimulus to a nerve does not increase the strength of the impulse passing down any one nerve fibre but merely leads to a greater number of nerve fibres being involved. It is corroborated by Gotch's observation that the electro-motive force in a stimulated nerve trunk always takes the same time to show itself and to reach its height and to disappear, however it be made to differ in amount by employing different strengths of stimulation.

Adrian's experiments point to the same conclusion. He finds that the time needed to narcotize a nerve trunk by alcohol or by other means, so that the passage of the nervous impulse is blocked, depends not merely on the length of the nerve trunk which is narcotized, but on the disposal of that length. For example, if in one preparation 9 mm. of nerve be narcotized, and if in another two lengths of 4·5 mm. of nerve be narcotized, these shorter lengths being separated by a non-narcotized interval of normal nerve, the times taken to narcotize one and to narcotize both of the 4·5 mm. lengths are the same, while they are considerably longer than that taken to narcotize the 9 mm. length. "The disturbance [corresponding to the nervous impulse] has much greater difficulty in passing one length of 9 mm. of affected nerve than it has in passing two lengths of 4·5 mm." We must assume "that the disturbance [corresponding to the nervous impulse] increases in size in the normal area between the two lengths of 4·5 mm." And Adrian brings forward evidence that "the increase of a subnormal disturbance on entering normal nerve tissue is certainly complete before the disturbance has travelled 5 mm. in the normal region, and it may be instantaneous"; and that "the disturbance increases to a fixed size on entering

¹ *J. of Physiol.* 1908-9, xxxviii. 113-133.

² *Ibid.* 1902, xxvii. 395-416.

³ *Ibid.* 1912, xlv. 389-412.

normal tissue no matter what its size may have been at the end of the region of decrement¹." For these reasons he concludes that the nervous impulse in the normal nerve fibre must obey the 'all or none' principle.

§ 5. So far as regards muscle and nerve. Their increase in *intensity* of function seems to depend on a greater *quantity* of elements (muscle fibres or nerve fibres) taking part in the action. With a weak stimulus only a few elements respond: with a stronger stimulus, other (less sensitive) elements are also involved. Each element follows the 'all or none' principle, which we have seen exemplified in the extensor thrust reflex.

How now in regard to sensations? Have we any sensations which behave similarly? Owing to the careful work of Head, Rivers and Sherren, the sensations afforded by the heat and cold spots of the skin can now be shown to afford an excellent instance of this behaviour². Different heat and cold spots are in different degrees sensitive to heat and cold. But each apparatus acts explosively and is easily fatigued. If the stimulus is strong enough, it reacts; if a still stronger stimulus is employed, it does not react differently. Hence we have here a clear instance of the 'all or none' principle. We have relatively sensitive and relatively insensitive heat and cold spots, and presumably these reflexly produce relatively considerable and relatively weak reactions. The differences in sensual intensity are correlated on the reflex level with differences in the strength of individual reactions. We may suppose that the nervous impulse from a more sensitive heat spot spreads centrally and hence efferently to a greater number of nerve fibres than are reached by the stimulus of a less sensitive heat spot.

§ 6. Thus the heat and cold spot sensations (and probably the other sensations belonging to the 'protopathic' system of sensibility), appear to be analogous instances of the 'all or none' principle. Each heat or cold spot, like the provocative of the extensor thrust reflex, gives a reaction which, within wide limits, is independent of the strength of the stimulus, provided that it is effective. But suppose these limits are overstepped. We have seen (page 141) that, with excessive increase in the strength (or with change in the character) of the stimulus, the extensor reflex suddenly gives place to the flexor reflex. So too, with like changes in the thermal stimulus, the quality of the sensation alters. The threshold of a new quality of sensation is overstepped: pain enters and quickly suppresses the temperature

¹ *Op. cit.* 399, 402, 412.

² *Brain*, 1905, xxviii. 105.

sensation¹. We cannot stay here to discuss whether the same end organs can give rise to pain and, with weaker strengths of stimuli, to heat, cold and touch. The important point now to observe is that the quality of the thermal or tactual sensation at once changes, just as the quality of the spinal reflex changes. A relatively innocuous 'exteroceptive' reaction, to use Sherrington's useful terminology, gives place to a 'nociceptive' reaction. And, just as in the case of the reflex, the pattern of impulse within the cord is different, so in the case of the sensation, the paths of the impulses as they pass up to the brain are (sooner or later) changed. At all events we have a hint that a difference of 'specific energy,' as it used to be called, may depend not (or not only) on peripheral conditions at the sense organ, but on more central conditions, say at the first synapses with which the impulses meet; in other words, difference of sensual quality may depend not merely on functional differentiation of end organs and peripheral fibres, but also on the mapping out of different patterns of response within the nervous system.

§ 7. But it will be objected that neither all reflexes nor all sensations obey the 'all or none' principle; and that what we have been considering are exceptions to the general rule. The question at once arises whether—inasmuch as muscle and nerve obey this principle, inasmuch as the extensor thrust reflex follows it, and inasmuch as the lowly protopathic system of sensibility shares it—we are not justified in assuming that higher reflexes and higher forms of sensibility are really governed by the same principle. Let us take, for instance, the 'scratch' reflex. Clearly this reflex, at first sight at least, disobeys the 'all or none' principle. The strength of the reflex can be graded according to the intensity of the stimulus. As the stimulus increases in strength, the clonic twitching movements of the scratching leg increase in amplitude, force, and number, although their frequency is practically unchanged. On the afferent side we may conjecture that more and more nerve fibres are being stimulated; on the efferent side that more and more nerve and muscle fibres are being stimulated. But what is going on at the spinal centres, that receive the afferent and emit the efferent impulses? May we not suppose that, shooting across the synapses, the disturbance preserves a constant 'type' of pattern of excitation, that pattern constituting a functional unit for evoking the reflex? When the stimulus is increased, the

¹ Cf. Rivers and Head, "A Human Experiment in Nerve-Division," *Brain*, 1908, xxxi. 381.

pattern of excitation changes. The additional afferent impulses shoot across into new synapses, which become integrated with the old. The old pattern becomes, so to speak, the nucleus of a more comprehensive pattern. But, despite these additions, the 'type' of the pattern is unchanged. The pattern functions in precisely the same manner as before, *i.e.* as a mechanism adapted for evoking the same kind of reflex. The central neural pattern changes, therefore, not in intensity but in extent; and it changes in extent in such a way that the type of reaction movement is still maintained. That even in graded reflexes the 'all or none' principle is followed is shown by the observation that the latent time taken to manifest the efferent effects of a sudden increase in an original afferent stimulation is practically identical with the latent time taken to produce the efferent effects of that original stimulation¹.

§ 8. Corresponding to the graded strengths of such reflex twitches we have, I suggest, such graded strengths of sensation as occur among auditory sensations. We can imagine that the reflexes caused by a tone of constant pitch of changing intensity vary in much the same way as the scratch reflex changes with strength of stimulus. The number of nerve fibres carrying the incoming nervous impulses increases with the growing strength of stimulation. Each nerve fibre follows the 'all or none' principle, while the central pattern, despite its growth, preserves its functional reaction unaltered in aim. So long as the pitch is kept constant, the type of reaction is practically constant, and hence the sensation is practically unchanged in quality. Further, the reaction, whatever its extent, preserves its character of indivisible integrity; for which reason, perhaps, an intensity cannot be analysed into component units of lesser intensity.

In certain respects, however, auditory sensations (indeed all forms of sensations other than protopathic) are distinguished from such a reaction as the scratch reflex, *viz.*, by the apparent absence of refractory periods, and by the virtual lack of fatigability. The movements of the scratching limb are clonic; they recur with a periodicity which is practically fixed and independent of the frequency of the stimuli applied to the afferent path of the reflex. An additional stimulus, inserted with the object of evoking an additional reflex movement interpolated between these movements, is ineffective. The reflex centre thus shows refractory periods, during which stimuli are inoperative. The reflex centre is also fatigable. But auditory

¹ C. S. Sherrington, *The Integrative Action of the Nervous System*, London, 1906, 24.

sensations, on the other hand, are not intermittent, there appears to be no refractory period; nor are they sensibly fatigable. A prolonged sound is heard uninterrupted; and it may be heard for minutes, days, or years, without appreciable alteration, so long as we choose to listen to it¹. The reasons for such continuity and indefatigability we shall examine more closely in the following paragraph. Having regard to their wide difference in complication, we should not expect too close a parallelism between the reflex spinal mechanisms and the far higher mechanisms concerned in sensation. It might be objected, too, that what holds on the 'physiological' side need by no means hold on the 'mental' side of a reaction. But this paper has been written from a diametrically opposite standpoint. Based on the hypothesis of psycho-physiological parallelism, it aims at indicating the light that may be thrown on the fundamental character of the psychical attributes of sensation by physiological considerations².

§ 9. There remain a third class of reflex, and corresponding to it a third class of sensation, which we have yet to consider. If we may describe the extensor thrust reflex as a 'gradeless' reflex, and the scratch reflex as a 'clonic' (or 'twitch') 'graded' reflex, we may term the remaining form of reflex a 'tonic graded' reflex. The reflexes, termed by Sherrington 'proprioceptive' reflexes, afford excellent instances of this form of reflex. They are based on the integration of a pair of antagonistic reflexes. The two centres of these reflexes are fed simultaneously by the same unceasing proprioceptive impulses, *i.e.* by impulses arising from within the body, especially from the muscles, tendons, etc., of the limbs concerned. These afferent impulses act reflexly so as to keep the muscles in a postural condition of perpetual tonus,—a condition which may be described as a state of active equilibrium of the double reflex. In this condition it rests as on a knife edge, from which it may be made to swing in one or other of two opposite

¹ If very weak, a continuous sound stimulus is only heard intermittently. (Into the responsibility of inhibitory processes or refractory periods for such fluctuations I cannot here enter.) If strong overtones are present, one or other of them may successively attract the attention (cf. C. Stumpf, *Tonpsychologie*, i. 361).

² A caution may be added against supposing that in the complete differentiation of sensations the efferent side of central reactions must necessarily show itself *visibly*, by altered muscular contractions. In the first place the efferent side of a reaction may lead not only to skeletal but to visceral movements; in the second place, not only to muscular but to glandular and other activity; and in the third place, not directly to outgoing peripheral activity at all, but to further central changes in virtue of the connexions of the main efferent limb of the reflex arc with other arcs.

directions, as between two poles. Broadly speaking, afferent impulses which cause (or are set up by) reflex *contraction* of a group of muscles governed by one centre of the double reflex simultaneously cause reflex *inhibition* of contraction in the antagonistic group of muscles governed by the other centre of the same reflex. It is the special rôle of such reciprocal graded inhibition to procure an exact adjustment between the strengths of incoming stimuli and outgoing discharges.

The tonicity of this class of reflex seems continuous and within certain limits indefatigable. The continuity, the lack of refractory period, probably depends on the phenomenon of after-discharge. The efferent impulses are not cut short (or inhibited) periodically as in the step reflex. They outlast the stimulus, and so, if the stimulus be repeated sufficiently often, a continuous instead of an intermittent reflex movement, a condition of 'posture,' results.

The tonic reflexes are especially prominent in the 'decerebrate' animal; the refractory periods, so characteristic of 'decapitate' and 'spinal' preparations, are suppressed. Decerebrate rigidity is entirely proprioceptive in origin; it is abolished by 'deafferentation'¹, and is produced by the action of the proprioceptive impulses on a central mechanism, situated above the spinal cord, these impulses abolishing the refractory periods which are so characteristic in the 'spinal' animal².

§ 10. Corresponding to this class of tonic graded reflexes, we have a class of tonic graded sensations, well exemplified (i) in the sensations of warmth and coolness obtained from the epicritic system of cutaneous sensibility and (ii) in the sensations of light obtained from the cones of the retina. Adaptation, contrast and indefatigability are the distinguishing characteristics of this class of sensation. Just as the extensor thrust reflex is fatigable, so we have seen the heat and cold spot system to be fatigable; just as neither the extensor thrust nor the scratch reflex can exhibit a condition of equilibrium, so neither the heat and cold spot system of sensation³, nor an auditory sensation (nor perhaps the retinal 'rod' sensations), is amenable to adaptation or contrast. There is no middle or 'indifference' point in heat, cold, or

¹ *I.e.* cutting off all afferent impulses.

² C. S. Sherrington, *Proc. Roy. Soc.* 1906, B. LXXVII.; *Quart. J. of exp. Physiol.* 1909; *J. of Physiol.* 1910, XL.

³ Cf. Rivers and Head, *op. cit.* 406-410. In point of fact, the scratch reflex is not a pure example of its class; the scratching leg assumes a definite 'posture,' besides executing a series of scratching movements.

sound intensity as there is in warmth and coolness, or luminous intensity. In place of fatigue we have, in the class of tonic graded sensations, a variable neutral point of adaptability, a 'tonic' sensation, which is neither warm nor cold, neither bright nor dark, from which it is possible to change in the direction of warmth or coolness, brightness or darkness, and to reach (within certain limits) a new state of adaptation. In place of two isolated mechanisms for heat and cold, we have an integration of the two 'incompresentable' polarities for warmth and coolness, within a single mechanism¹.

§ 11. In the case of the class of tonic graded sensations, it is far more difficult to discriminate between changes in intensity and changes in quality. Who shall say whether the gradations of the warm-cool (or white-black) series of sensations change in intensity or in quality?

Nor is the confusion merely between intensity and quality. With the class of tonic graded sensations a new element enters, that of 'extensity.' One sensation appears to have more extensity than another; it appears to come from a wider sensory area. But, as on the spinal reflex level, if the same stimulus is applied to a wider sensory area the sensation must alter at the same time in intensity and often in quality. A vessel of warm water, in which the whole hand is immersed, feels warmer than when a single finger is introduced. The intensity and hue of a luminous sensation depend not merely on the strength of the stimulus at the point of stimulation but also on the area, the number of points, of application of that stimulus.

So close a connexion between extensity and intensity has been supposed to be correlated anatomically with the mode of termination

¹ How sensation, like a reflex, leaps from plane to plane has been already exemplified (p. 143) in the passage from heat, cold and touch to pain. The plane of the protopathic system is in turn distinguished, as we have just mentioned, from that of the epicritic system of cutaneous sensibility by its gradation, by its power of adaptation, by its (relative) indefatigability, and, further, by its freedom from diffusion and radiation and by its power of accurate localisation and of estimating relative size. So, too, the vibrations of a tuning-fork applied to the skin (*a fortiori* when applied to the ear) evoke a central reaction different from a mere series of touches. Thus Head and Holmes record a case of Brown-Séquard paralysis in which "the vibrations of a tuning-fork were appreciated badly or not at all over the right arm and leg, in spite of the complete integrity of tactile and pressure sensibility" (*Brain*, 1911-12, xxxiv. 111). So, too, a flickering light, as soon as the rapidity of the flicker is so great as to produce an uninterrupted sensation, establishes a new sensation, behaving, as Sherrington has shown (this *Journal*, 1904, i.), very differently from a series of flicker sensations; that is to say, a new 'type' of central neural pattern is at once initiated.

of afferent nerve fibres at the periphery. Any given fibre divides into fibrils and supplies a relatively wide sensory area, which is also supplied by similar divisions of other neighbouring fibres on either side of the given fibre. Hence it has been thought that extending the area of stimulation involves stimulation of a greater number of fibrils of the same fibre, and so leads to increase in the intensity of the nervous impulse along the nerve fibre. But if, as we have seen reasons for believing, nerve fibres act according to the 'all or none' principle, this explanation of the connexion between extensity and intensity falls to the ground, and we must seek some other—preferably in the close similarity of reaction, *i.e.* in the partial identity of neural pattern, which a more intensive stimulus and a more extensive stimulus reflexly call forth. Both the extensive and intensive series affect a greater number of nerve fibres as they are increased; both therefore call forth at first almost the same central changes of neural pattern, and almost the same efferent changes in outward reaction.

§ 12. We have seen that, so long as the reaction is of the same 'type,' pursuing the same plan and purpose, the corresponding sensation alters only in intensity; but that, when it begins to alter in type, differences of quality make their appearance. It is not surprising, then, that when sensations are made to increase in intensity, they so often show more than changes in mere intensity. Indeed it is probable that no sensation can be increased in intensity without at the same time undergoing some change in quality.

But this consideration alone hardly helps us to understand why the warm-cool and bright-dark series of sensations approach so much closer to changes in quality than do those belonging to the other two systems of sensation we have considered. We have also to take into consideration the manner in which the qualities of a given stimulus are differentiated in the evolution of sensibility. We start, I think we may assume, with a vague 'whole' of sensibility, which is differentiated into an increasing number of constituent 'parts.' Auditory stimuli of different vibration-frequencies at first give rise to a vague appreciation, merely perhaps of high and low pitch; we come ultimately, by superadded reactions, to have an enormous number of different types of reaction corresponding to different vibration-frequencies of stimulation¹.

In the case of colour sensations, it is, I think, generally agreed that they have been differentiated from the colourless series of sensations.

¹ Cf. H. J. Watt, this *Journal*, 1911, iv. 146.

But even prior to such differentiation, I venture to conceive a time when the various colour stimuli produced *vague* sensations of colour and of white (evolved from the still more primitive phenomena of taxis) by their action on three different neural centres. These centres, each with its appropriate pattern, correspond respectively to red, blue and yellowish-green sensations when excited individually, and to orange, yellow, purple, etc., or to white when two or more of them are excited simultaneously. At such a stage we may suppose a fairly close correspondence between visual and auditory sensibility. The reactions of both are due to the excitation of a group of *single* centres. Any one neural pattern is variable in a single direction only, the extent of variation corresponding to the degree of sensual intensity.

In this system of three-colour sensibility sensual contrast, adaptation and brightness are unknown¹. Upon it a more complex system of sensibility, giving rise doubtless to new and 'higher' sensual qualities, has been erected. All colour stimuli come now to act on *double* centres corresponding to reflexes of the tonic graded type and giving rise, in the first place, to a series of sensations of graded brightness. Brightness replaces intensity now that the reaction swings from pole to pole, instead of, as before, from zero to the maximum of reaction. The corresponding sensations vary from white through grey to black and are characterized by contrast, adaptation and lack of fatigability.

From within this paired system of colourless sensibility two similar paired systems of colour sensibility become differentiated. The yellowish-green component of the lower three-colour system is now divided into yellow and green elements, and the red and green form the one, while the yellow and blue form the other, of the two new paired centres. It is as if from an axis of up- and down-reactions (corresponding to the white-black series of sensations) there had arisen two opposite to- and fro-reactions, each in a plane at right angles to the original up- and down-reaction (one to- and fro-reaction corresponding to the red-green, the other to the yellow-blue series of sensations). We must suppose that each colour stimulus acts not only on the white-black but also on the red-green or yellow-blue system (or on both systems) of sensibility. We must further suppose that when either of these two colour systems of sensibility is in equilibrium there is

¹ Of course contrast (and adaptation) of a sort are never absent; there is a contrast effect in a heat-followed-by-cold experience, or in a noise-followed-by-whisper experience. But such instances differ not in degree but in level from the sensual contrast (and adaptation) we are here considering.

no sensation save the grey which results from the ever simultaneous action of the colour stimulus on the white-black system.

Thus we are able to start from a (variable) neutral balancing point of equilibrium, a state of tone, from which we may progress in one direction or the other, from any grey either towards red, green, yellow or blue (or any intermediate shade of colour). Or, perhaps largely in virtue of the more primitive three-colour system, we may start from red and proceed to green by orange and yellow, or by carmine and violet. This mode of evolution of the colourless and colour series of sensation may help us to understand our difficulty in determining whether gradations in either series represent variations in quality or intensity. Quality and intensity appear to meet here; it becomes as difficult to be certain that, as we pass from red through shades of orange to yellow, we are passing through changes not of intensity but of quality, as it is difficult to be certain that, as we pass from black through shades of grey to white, we are passing through changes not of quality but of intensity. The same holds good for the evolution of our sensibility to warmth or coolness, although here (and possibly also in vision) the co-existence or the precedence of a more primitive 'spot' system of varying sensibility may have been psychically helpful. As Sherrington has said of the corresponding reflexes,—propriospinal reflexes normally fuse with other reflexes as adjuvant to them.

The close connexion we have traced between the colourless and the colour series of sensations is also shown in our ability to express the latter in terms of the former, *i.e.* to estimate in terms of greyness the *brightness* of a colour sensation. The *intensities*, on the other hand, of two very different colours we can only vaguely compare, either by their physiological action (so we may vaguely compare the intensities of two tones of very high and very low pitch), or by objectively imagining what amounts of colour there are in the respective stimuli (so we may vaguely compare the intensities of two different qualities of gustatory or olfactory sensation).

Our neurological considerations have led us to trace our experience of intensity back to a stage when it was possible only by a comparison of experiences corresponding to the neural patterns of two or more quite separate sense apparatus. At this first stage there were in question but two opposite qualities, two types of reaction,—one, *e.g.*, a heat reaction obtainable from scattered heat spots, the other a cold reaction obtainable from scattered cold spots, of varying sensitivity.

At this stage the gap between heat and cold sensations was not bridged.

In the second stage of sensibility, we find both quality and intensity far more advanced. Not only can intensity within a given sense apparatus be delicately graded, but also quality admits of further gradation by the simultaneous excitation of two or more elementary reactions in different degrees. Thus, given two colour qualities red and yellow, every shade of intermediate quality is obtainable by simultaneously presenting the two stimuli in different strengths.

In the third or last stage, quality is still further developed; it rises into 'modality.' It is no longer possible to pass from one quality (*e.g.* bitter, red, or warm) to another (*e.g.* sweet, green, or cool, respectively) without passing through a quality which belongs to neither. Such antagonistic qualities, 'modalities' as Helmholtz called them, are inco-presentable; when simultaneously excited, they either 'neutralise' one another (giving rise to a new sensation) or show rivalry. What remains of intensity at this stage is a relic from the second stage in the evolution of sensibility, when we are dealing with one (or more) single centres in action, each starting from zero; whereas at this third stage we have one (or more) double centres in action, inclining to one or other side of a variable point of active equilibrium.

III.

The conclusion, to which we have been led, is that the ultimate difference between the quality and the intensity of sensation depends on the nature of the underlying reaction. Broadly speaking, when the reaction changes its fundamental type, it alters in quality and the sensation changes also in quality. So long as the reaction preserves its fundamental type, it can be said to vary only in quantity, and the sensation changes also in intensity.

In this sense sensual intensities are quantitative. But intensities are *not* quantitative in the sense that there is a moreness or lessness of excitation within the same anatomical area; for we have seen reason to believe that any given neural tissue, central or peripheral, follows the 'all or none' principle. Nor are intensities quantitative in the sense that the stronger sensation *contains* the weaker, as a large quantity may be said to contain a smaller; this is obviously contradicted by introspective experience. But in so far as they fail to answer to these requirements, it does not follow that sensual intensities are qualitative,

unless we merely mean that they resemble sensual qualities in behaving differently from quantities (which after all are not in the same sense sensual).

We cannot even say that intensities are quantitative, merely in the sense, and for the reason, that they show a moreness or lessness of the same sensory character. Otherwise we must term the differences in any series of colour sensations, ranging say from red to yellow, differences of intensity. We can speak of colour sensations as differing in moreness or lessness of hue; we can describe olfactory or auditory sensations as having more or less of a certain quality. But we do not in general confuse such variations in quality with variations in intensity, of the sensation.

Sensual intensity thus means, at bottom, *how much* reaction; sensual quality means *what type* of reaction. That is to say (if only we can legitimately translate neurological into psychological process) intensity and quality of sensation are dependent on variations of mental activity in one or other of two directions¹.

Now when the present activities of higher systems look back on the past activities of higher systems, we get a differentiation of the activity consciousness into consciousness of mental 'processes' or 'acts,' on the one hand, and of mental 'products' or 'contents' on the other. If the lowly sensual levels could but look back upon themselves, their activities would be similarly differentiated into 'acts' of sensing and 'contents' of sensation. But this is not possible. We experience 'acts' of apperception, thinking, willing, imagining, etc., in all of which the self is involved; but we have no experience of the 'act' of sensing. It is true that intensity and quality are *derived* from the activity of the sensual level. But they come to us as contents of consciousness, not merely by virtue of such lowly activity (itself undifferentiated into act and content), but also by virtue of our possession of higher and still higher forms of experience culminating in comparison, relation and abstraction. We are able to compare and to relate individual reactions differing in 'moreness' or 'lessness' or in 'type'; and thus finally we reach the abstract forms (the *Gestaltvorstellungen*) of intensity, quality and quantity generally².

¹ That these directions are fundamentally different is shown by the limitation of Weber's law to intensities.

² This is well shown in certain parietal cortical lesions, where our powers of unconscious comparison, relation and attention seem at fault, and in consequence not only the differential, but also the absolute, thresholds of sensations (as judged by the uniformity of correct answers) suffer, although sensibility itself may be relatively little affected (H. Head and G. Holmes, *Brain*, 1911-12, xxxiv. 102-254).

If pressed to give a definite answer to the question whether intensity differences are quantitative or qualitative, I decline to be bound to either horn of the dilemma. To me the question seems *mal posée*, since it is capable of every conceivable answer. Thus it is open for anyone to call a change in sensual intensity qualitative,—in so far as it is not directly measurable; in this sense, a change in any other sensual attribute (in quality, extensity or protensity) is also qualitative. Or a change in sensual intensity may as legitimately be called quantitative,—in so far as (even like changes in sensual quality as contrasted with sensual modality) it depends on ‘more or less’ of a given complex of reaction, and in so far as (even like changes in sensual quality or modality) it is indirectly capable of measurement in spatial and temporal terms based on sensual extensity, sensual protensity and movement. On the other hand, since changes in sensual intensity occur in a specific direction quite different from that of quality changes, they may be called non-qualitative. Indeed it is thus arguable that intensity differences are neither qualitative nor quantitative, but strictly *sui generis*, *i.e.* intensive.

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ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE? II.

BY G. DAWES HICKS.

- § 1. *The relation of qualitative and quantitative.*
- § 2. *The physiological correlate of differences in quality and quantity of sensation.*
- § 3. *Some general points of psychological theory.*
- § 4. *Psychological criticism of Dr Myers's hypothesis.*
- § 5. *Bergson's explanation of the reason why we regard sense contents as quantities, and a criticism of that explanation.*
- § 6. *Meinong's account of intensive quantity.*
- § 7. *Meinong's interpretation of Weber's Law, and criticism thereof.*
- § 8. *Is quantitative comparison always an act of judgment?*
- § 9. *Differences of intensity may be said to be magnitudes but not quantities.*

§ 1. WITH many of the concluding observations in the preceding paper I am in accord. I agree that the question whether intensities are qualitative or quantitative is badly framed, and admits of no intelligible answer. "There is," as Mr Bradley puts it, "no such thing as quantity *merely* extensive, or as quantitative differences without quality. Because anything is qualitative, that is no reason why it should not also have quantity¹." The quantitative, in other words, is but an abstract aspect of what we are actually dealing with even in that region where its importance and significance are indisputable, and quantitative explanation has always the perfection and the imperfection which attach to abstract treatment. Whatever be its nature, the material world can never find complete expression in quantitative terms. It has, and is bound to have, its own structure or collocation of parts. Though we divest the parts of every shred of qualitative distinctness, though we reduce them to what can be satisfactorily

¹ A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the Mind Association, in London, 7 June, 1913.

² *Mind*, N.S. iv. 5.

rendered in quantitative formulae, yet, however far we push this method of procedure, there will always remain at least one qualitative element that cannot be eliminated,—the particular *kind* of distribution which holds good at any given moment. Any such absolute separation as that which Münsterberg¹ would institute between the physical and the psychical, quality being excluded from the one no less rigidly than quantity is excluded from the other, is bound, therefore, to lead to a thoroughly false conception of the way in which the two disparates are related the one to the other. I agree, also, with the remark made in an earlier part of the paper (II, § 12), although I think it not a little damaging to the author's main thesis, that probably a sense-presentation can seldom be increased in intensity without at the same time undergoing some change in quality. Particularly in visual apprehension, the sense-experiences we have to deal with are so complex, the factors implicated are so numerous, that it is hardly conceivable there should ever be changes in intensity only with qualitative constancy.

Further, though the considerations which Dr Myers has adduced should appear inadequate to bear the weight of the hypothesis he would base upon them, few will question the value and interest of those considerations in themselves. Whatever else Münsterberg succeeded in doing in the third of his *Beiträge*, he certainly managed to make manifest the important function fulfilled by kinaesthetic factors in the development of sense-experience, and Dr Myers has accomplished a most useful piece of work in the same direction by showing how from the physiological side those factors call for recognition. He has made it abundantly evident that the prevailing tendency to interpret sensation as though it were a purely cognitive process is a mistaken tendency, and that the complexity of the total process, as involving not merely awareness of a content, but a change in the state of feeling, and consequently specific forms of movement, requires to have more justice done to it.

§ 2. I am not, however, convinced that a case has been made out, even from the physiological side, for supposing that "the ultimate difference between the quality and the intensity of sensation depends on the nature of the underlying reaction." Dr Myers, it will be noticed, passes over with very scant mention the peculiar structure of the end organs and the mode of their development. With "the mapping out of different patterns of response within the nervous system," he does, it is true, in one place (II, § 6) couple the "functional

¹ *Grundzüge der Psychologie*, I. 260 sqq.

differentiation of end organs and peripheral fibres" as a condition upon which the difference of sense quality may in part depend, but in the rest of his investigation the latter seem to disappear from view and exclusive prominence to be given to the former. He is in no way wishful, I take it, to minimise the significance of the elementary fact that the physical stimuli themselves are different, that, for example, ether waves and air waves are essentially dissimilar, nor to suggest that the gradual differentiation of the sensory mechanism has not been largely determined by these external dissimilarities. But what I miss in his treatment is any indication of the way in which he conceives the two factors, (*a*) the mapping out of different patterns of response within the nervous system, and (*b*) the functional differentiation of end organs and peripheral fibres, to be connected with each other. The argument certainly does seem to require that the latter be placed in a position of such decided dependence upon the former as is hardly reconcilable with the biological evidence. The broad general fact that all the organs of special sensation are originally derived from the ectoderm would seem to point in the opposite direction, and the rudimentary stages of (say) the invertebrate eye,—the gradual formation, namely, of groups of pigmented cells,—appear to be naturally explicable from the action of the physical stimulus upon the epithelium. Moreover, I note that a significant change in the presentation of the case with reference to the 'all or none' principle occurs when Dr Myers advances from muscle and nerve to sensation. In respect to the former, the rule is laid down that with a weak stimulus, only a few elements or fibres respond, whilst with a stronger stimulus, other elements or fibres are also implicated. But in respect to thermal sensations, allowance has to be made for the fact that the heat and cold spots are relatively sensitive and relatively insensitive, so that "presumably these reflexly produce relatively considerable and relatively weak reactions," and "we may suppose that the nervous impulse from a more sensitive heat spot spreads centrally and hence efferently to a greater number of nerve fibres than are reached by the stimulus of a less sensitive heat spot" (II, § 5). That is to say, when we take not any one heat (or cold) spot, but any group of such spots, there is a gradation in the intensity of the corresponding sensations, only the important factor then is not increase or decrease in the strength of the stimulus, but the relative sensitiveness or insensitiveness of the end organs concerned. In other words, there has to be recognised at the very outset, so far as sensations are concerned, the essential dependence

of their intensity upon peripheral conditions. Equally so is it in respect to their quality. Dr Myers argues (*ibid.*) that just as with excessive increase in the strength of the stimulus, the extensor reflex suddenly gives place to the flexor reflex, so with like changes in the thermal stimulus, the sensation alters in quality and becomes a sensation of pain. Curiously enough, however, he passes over the question, which is surely relevant to his argument,—“whether the same end organs can give rise to pain and, with weaker strengths of stimuli, to heat, cold and touch.” If different end organs come here into play, and there seem strong reasons for thinking this to be probable, the parallelism which it is sought to establish with what happens in the case of the extensor thrust reflex so far breaks down.

§ 3. The criticism, however, which I am concerned to press is criticism of a psychological kind. At the outset, I had better perhaps refer to certain general points of psychological theory in regard to which, if I understand him rightly, I should dissent from Dr Myers. For the sake of brevity, I group them under the following five heads:—

(a) We are at one as to the necessity of distinguishing in what is called a ‘state of consciousness,’ the act or process of consciousness from the content of consciousness. But this distinction I take to be a distinction of aspects, and not a distinction of two existences. Act and content are not, I should say, what, for example, the Herbartian psychologists inclined to conceive them as being, two independent and separate entities. The distinction does not, therefore, seem to me to be accurately described as involving “two main divisions of consciousness, —the consciousness of ‘acts’ or ‘processes’ and the consciousness of ‘contents’ or ‘products.’” On the one hand, ‘acts of consciousness’ are not necessarily ‘the consciousness of acts.’ Constituting as they do the very life of the conscious subject, they are rather ways *in* or *through* which that subject is aware than objects *of* which he is aware. On the other hand, the content, although fairly enough in one sense called a product, is not necessarily a product in the sense of being wholly a construction on the part of the mind itself. And it may be, I think it is, an error to give to the content the position of an independent object, and to picture the act of consciousness as a sort of inner vision directed upon it.

(b) Although the duality of act and content be involved in the most rudimentary phases of the mental life, it does not by any means follow that even the crudest *recognition* of that duality is a primitive or

primordial fact of mind. To differentiate, for example, hearing from the sound heard implies, I should say, an experience far more complicated and the use of concepts far more abstract than we can ascribe to the animal consciousness. But this does not, I think, constitute a fundamental severance between sense and the other cognitive processes, as Dr Myers seems to suggest. I cannot discover any ground for saying that in the mature mental life there is no recognition of the distinction between the act of sensing and the content sensed, any more than I can find ground for saying that in the less mature mental life there can be no imagining or apperception before the corresponding recognition is reached.

(c) Those who lay stress upon the part played by sensations of muscular strain or tension in the so-called 'feeling of effort' do not necessarily deny that consciousness is an activity, nor even that there may not be awareness of such activity. What they do call in question is the legitimacy of identifying *that* activity with the activity of which there is awareness in the 'feeling of effort.' Certainly to me it seems doubtful whether we can ever be said to be directly aware of the activity involved in consciousness itself,—aware of it, I mean, after the manner in which we are directly aware of a colour when we actually see it or of a sound when we actually hear it. But, in any case, I should be prepared to maintain that the 'feeling of effort' evinces itself as the outcome of a long repetition of experiences, and as having acquired an appearance of simplicity which disguises from us its really complicated character.

(d) It is not precisely clear what Dr Myers means by the possibility of "a choice on the part of the organism between two or more reactions to a given stimulus" as the condition of the appearance of consciousness (II, § 1). But if he intends to suggest that conscious choice, though it may be of the crudest kind, is an essential factor of mental life, I should hesitate in following him. It cannot be supposed that the consequences of specific reactions are in any way prefigured or represented in anticipation by the primitive consciousness. And until that in some vague way comes about, I do not see what conscious choice can mean.

(e) I do not think we are entitled to lay down as a psychological presupposition that "differences in type of movement must be the cause of differentiation in the quality of sensation," on the ground that "it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable

in promoting different types of response." With the appearance of conscious life, advantageousness for the organism may not be the sole principle determining the course of evolution and may become less and less so as mental evolution proceeds.

§ 4. I return then, now, to the hypothesis propounded by Dr Myers. According to that hypothesis, if I interpret it correctly, the determining factors throughout, in regard both to the quality and the intensity of sensation, are the nervous responses and the efferent reactions which ensue upon the respective stimuli. The lower and the higher forms of sensibility, not less than the lower and the higher kinds of reflexes, are in reality governed by the 'all or none' principle. Within certain limits the type of reaction remains the same, and the grading of the latter in strength according to the strength of the stimulus (when it is so graded) means that as the strength of the stimulus increases more and more nerve and muscle fibres are called into play, the corresponding sensation meanwhile altering only in intensity. If, however, those limits be exceeded, if the increase in strength of the stimulus be excessive, the type of reaction changes, and a difference of quality makes its appearance in the corresponding sensation. Apart from the admittedly conjectural character of most of the essential steps of the argument, the objections I feel inclined to urge are these. In the first place, it is not easy to gather from Dr Myers's exposition in what relation he conceives the sensation to stand to the reaction. Which is the antecedent, and which the consequent? The whole trend of the line of thought pursued would seem to imply that when it is said that "the ultimate difference between the quality and the intensity of sensation depends on the nature of the underlying reaction," the dependence intended is a chronological dependence, and that in the order of sequence the sensation follows the reaction. But can it be maintained that this is in accordance with fact? And if it can, what becomes, on such a supposition, of the contention that "it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable in promoting different types of response" (II, § 1)? If, on the other hand, the sensation precedes the reaction; if one sensation "leads to" one type of reaction, and another sensation to another type, then how can we "attribute the intensities of sensation to different degrees of the same reaction" (II, § 2), how can the circumstance that a sensation is unchanged in quality be due to the fact that the type of reaction is constant (II, § 8)? In short, how is the conception

of different qualities of sensation *promoting* different types of response reconcilable with the doctrine of "psycho-physiological parallelism," upon which Dr Myers tells us his view is based? In the second place, I fail to see in what way the hypothesis helps towards the solution of the problem which is the subject of this discussion. Suppose it be true that the correlate of differences in quality of a sensation consists in differences in type of reaction, and that the correlate of differences in intensity of a sensation consists in different degrees of the same reaction. Still, even then, the reaction forms no part of the content of the corresponding sensation, and *for the experiencing subject* no comparison of this content with the reaction is any more possible than with the external stimulus. Howsoever it may be for the scientific reflexion of the physiologist, for the experiencing subject sensual intensity most assuredly does not mean how much reaction, nor does sensual quality mean what type of reaction. There is nothing in the redness of red or the blueness of blue or in the sound of a tone heard to suggest to the experiencing subject specific types of nervous and muscular reaction, nor is there anything in the varying intensity of a colour, or in the faintness or loudness of a note, to suggest to him a greater or less number of efferent nerve and muscle fibres in a state of stimulation. And the question as to the intensity of sensations has to do, I presume, primarily with sense contents,—whether, namely, they warrant any definite assertions as to difference of amount, or whether what are taken to be differences of amount do not, in truth, indicate solely qualitative changes. I do not find that the answer to that question is materially furthered by correlating sense presentations with types and degrees of response within the nervous system rather than with kinds and amounts of external stimulation. When worked out from the psychological side, Dr Myers's theory would, I imagine, lead him, in the long run, to a conclusion resembling that of Münsterberg in the *Beiträge*, according to which muscular sensations, or sensations of strain (*Spannungsempfindungen*), mediate as a *tertium quid* between the physical and the psychical. Sensations proper, in Münsterberg's view, vary only in quality, and what is usually called a more intense sensation is, in all cases, a content of consciousness numerically and qualitatively distinct from the weaker sensation with which it is compared. Muscular sensations, on the other hand, occupy a unique position in the mental life; although sensations, they share with physical entities the characteristic of varying only in quantity and not in quality. Since

any physical stimulus necessarily evokes a change in muscular strain or tension, the equivalent of which in consciousness is a *Spannungsempfindung*, all sensations proper have as their accompaniments muscular sensations, and these afford the basis of measuring differences of intensity. Some such mode of translation into psychological terms the theory before us would appear to demand, and Dr Myers himself almost hints as much when he insists at the beginning, that "the very word intensity means a state of tension or strain" (I, § 3)—a remark, by the way, which seems to conflict with the concluding suggestion that "intensity differences are neither qualitative nor quantitative, but strictly *sui generis*." Ingenious, however, as Münsterberg's treatment of the problem undoubtedly is, it raises, I think, more perplexities than it succeeds in removing. The peculiar character ascribed to muscular sensations, as *toto genere* distinct from the character of all other sensations, is eminently unsatisfactory, and no serious attempt is made to show how it comes about that in comparing the intensities (say) of two sounds, our judgments are directed upon the sense-presentations themselves, and not upon their assumed concomitants¹. Perplexities of a like kind would, I am persuaded, confront Dr Myers the moment he attempted to specify the psychological equivalents of the physiological factors which he takes to be involved in the differentiation of sensual quality and intensity. I am far from wishing to dispute the contention that kinaesthetic sensations are implicated, in some form or another, in every mode of sense experience. I think it likely enough that they are. But, after all, sensations of tension and strain have their own content, and from that content to the neural responses and muscle reactions themselves is a far cry. It seems to me that we have here a problem thrust upon us precisely the same in character as that which presented itself when the correlatives were taken to be the external stimulus and the sensation.

§ 5. Somewhat similar obstacles beset the path of those who, like Bergson, maintain that the contents of mental states cannot rightly be

¹ The stress of these and allied difficulties probably occasioned the change of attitude observable in the *Grundzüge*. At all events, in the later work, Münsterberg disputes apparently the possibility of any, even an indirect, measurement of sensual intensity, and what he has to say about muscular sensations deviates markedly from his earlier mode of dealing with them. See *Grundzüge*, I, 263 *sqq.* Note, especially the remark on p. 280, "Spannung und Streben bedeutet also Kraft für die vorpsychologische Wirklichkeit und für die empirische psychophysische Persönlichkeit, im System der psychologischen Bewusstseinsinhalte bedeuten sie dagegen nur eine Erfahrung und stehen dem Probleme der messbaren Wirkungen nicht näher als die Empfindungen blau und tönend und sauer."

treated as magnitudes, that the relations of greater and less are not properly applicable to them. Bergson admits that ordinarily we do, without the slightest hesitation, pass judgments involving quantitative comparison upon the contents of our experience. He has, then, to explain how it comes about that into the field of what is purely qualitative the appearance of intensive magnitude intrudes and creates the illusion of progressive increase and decrease. The explanation is obtained by tracing back the appearance to the natural propensity of consciousness to objectify mental states, to regard them, that is to say, as extensive *quanta*. In the case of what he calls representative states, we transfer he thinks the idea of the cause which is quantitative into the effect which is purely qualitative, and the notion of intensive magnitude is only a "perception acquise"; in the case of affective states, we give the name of intensity to the larger or smaller number of sensations which we associate with the fundamental sensation, and the notion of intensive magnitude is here a "perception confuse." When, for example, we experience a pain which becomes, as we say, more and more acute, consciousness distinguishes, within the characteristic sensation which gives the tone to all the others, a larger or smaller number of sensations arising at different points of the periphery, muscular contractions, organic movements of various kinds, and the totality of these elementary psychical states expresses the new exigencies of the organism in presence of a new situation thus constituted for it. We estimate the intensity of the pain by connecting the differences of sensation with the reactions which usually accompany them, and which are more or less extended; by prefiguring, that is to say, the future bodily movements in the very midst of the sensation which is being experienced. When, again, we estimate quantitatively the loudness of a sound, we take into account not merely the change or disturbance in the vital condition of the organism, but also the fact that, by striking some object and thus expending a definite quantity of effort, or by exerting ourselves in the use of our vocal organ, we have repeatedly produced a similar sound; and the idea of this effort immediately presents itself when we transform the intensity of the sound into a magnitude¹.

There are, indeed, certain portions of Bergson's analysis which seem to me to be entirely on the right lines. In dealing, for instance, with the so-called "sense of effort," he complains, not without reason, of the crudeness of the conception of "a psychic force imprisoned in the mind

¹ *Les données immédiates de la conscience*, Ch. 1.

like the winds in the cave of Aeolus, and only waiting for an opportunity to burst forth," and of the will as watching over this force, and from time to time opening a passage for it. The considerations which he urges in favour of regarding experienced "effort" or "activity" as a content of consciousness, and not as itself identical with the activity of consciousness, although perhaps not in themselves sufficient to establish this conclusion, can, when reinforced by others which he might have used, be formed into a coherent body of evidence which it would be extremely hard to resist. To isolate the act of apprehending from the content apprehended, and to attribute to the latter a strength or intensity of its own, which may vary independently, is a procedure for which no justification is yielded, so far as I can discover, from psychological analysis. At all events, we are, I think, entitled to say that the differences of intensity which we discriminate in the content apprehended are not to be regarded as equivalent to a greater or less amount of apprehending activity. We by no means of necessity apprehend the more intense better or more accurately than we apprehend the less intense. We are more liable to overlook changes of loudness in the roar of a cannon shot than those in the buzzing of a bee. Leaving on one side the thorny issue whether, as it is misleadingly stated, the intensity of a sense content may be increased by attention, I would only insist that, in any case, such a definition of attention as "the variously related degrees of psychic energy expended upon the different aspects, elements, and objects, in the one field of consciousness"¹ prejudges at the start the fundamental question which it is the very business of psychological investigation to decide. For the increase of clearness and definiteness which results from attention may depend not upon a "focussing of psychical energy,"—a conception which we shall try in vain to render intelligible,—but upon the number and kinds of discriminations we are able to make in the content attended to, the distinguishable marks we are able to recognise in it,—a process which would consist largely in connecting the said content with, in relating it to, representations and ideas already possessed by the apprehending subject². Clearness, certainly, is one thing, and intensity another, but if through attending the content becomes more intense, an explanation of that circumstance must be found that is consistent with the explanation we are enabled to give of the increase in clearness and distinctness.

¹ Ladd, *Psychology, Descriptive and Explanatory*, 74-5.

² Cf. my paper on "The Nature and Development of Attention," in this *Journal*, 1913, vi. 1.

Bergson's arguments relate chiefly, however, not to the processes of consciousness but to the contents apprehended thereby, and in this reference the explanation he has to offer seems to me to fail. The failure evinces itself, I think, in much the same manner whichever be the department of experience with which he is dealing. Take, for instance, his account of the way in which we come to regard a pain as increasing in intensity. He recognises that it will not do to say that the more intense pain corresponds to a greater nervous disturbance, for these disturbances are as movements unconscious, and their equivalent in consciousness has no resemblance whatsoever to motion. But, he contends, the automatic movements which tend to follow the stimulus are likely to be conscious as movements, and the differences of sensation are interpreted by us as differences of quantity because we connect them with the reactions which usually accompany them and which are more or less extended. Now, the obvious question which at once presents itself is, why should the movements that accompany the sensation be said to be unconscious, and those that follow it be said to be conscious, as movements? It becomes very soon apparent that the latter way of speaking is no more than a metaphor. By movements that are conscious as movements Bergson here means simply the sum of sensations that arise from muscular contractions, organic conditions, changes in the state of joints, tendons and skin, and so on. In other words, the apprehension of these movements as movements is just as distinct from the fact of movement itself as are all presentations from the objective events giving rise to them. The factors called in to account for the appearance of intensive magnitude ought, *ex hypothesi*, then, to be as little capable of yielding it as the sensation of pain itself. Moreover, the theory is not, I think, confirmed by the appeal which is made to experience. When I become aware that a tooth-ache from which I am suffering is becoming more acute, or that a headache is becoming more severe, I fail to detect even by the most careful introspection any reference at all to the "thousand different actions" I might take in order to avoid either of these calamities. What introspection does seem to testify is that the estimate of intensity is derived directly from the experience of the pain itself. Now, I quite admit that introspection may in this respect be deceptive, but if it is, the deception stands in need of explanation, and the difficulty of finding one along the lines that have been followed seems well-nigh insuperable. Finally, the criticism I am urging may be summed up in more general terms. True though it may be, that in mature experience our judgments as to the comparative

intensity of two sense contents are constantly aided by the knowledge we possess of the physical world, yet it is impossible to suppose that such judgments could have become possible, if ultimately such sense contents are never directly apprehensible as standing to one another in a relation of greater and less. What problematical knowledge of the cause of olfactory sensations could conceivably have originally induced us to pronounce one smell to be stronger than another, if the contents of those experiences did not themselves furnish the data for such comparison? Our scientific modes of exact measurement, our interpretation of the physical world in quantitative terms, itself presupposes psychologically the more rudimentary comparison between the contents of sense experience. Doubtless, such scientific knowledge when attained facilitates and modifies the judgments we form of the increase and decrease of sensual intensities, but in no case can the latter be wholly dependent on that knowledge.

§ 6. "The fact is," says Bergson, "that there is no point of contact between the unextended and the extended, between quality and quantity. We can interpret the one by the other, set up the one as the equivalent of the other; but sooner or later, at the beginning or at the end, we shall have to recognise the conventional character of this assimilation¹." The abstract severance thus formulated is the basis upon which the refusal to recognise intensive magnitudes is rested. Extensive magnitude, so the argument runs, involves the relation of container to contained, involves, in other words, the relation of whole and part, and from the point of view of magnitude, there could be nothing in common between the extensive and intensive, save the divisibility which the relation of whole and part implies. But, since intensive qualities are indivisible, to speak of them as magnitudes is a contradiction in terms.

In his elaborate essay, *Ueber die Bedeutung des Weber'schen Gesetzes*, Meinong does not explicitly refer to Bergson's treatment of the subject, but the essay contains what is still by far the most complete and conclusive answer to the points that Bergson raises². I note some of the main features of Meinong's argument.

The first important fact he seeks to establish is that there are

¹ *Les données immédiates de la conscience*, 52. I do not know how the contention in *Matière et Mémoire*, 242 sqq. et passim, that "all sensations are primarily extensive," is to be reconciled with the sharp antithesis that is drawn in the earlier work, nor how the non-quantitative character of sensations is to be sustained in the face of that contention.

² The second volume of Meinong's *Gesammelte Abhandlungen* (Leipzig: Barth), 1913, has just reached me. It contains the essay on Weber's Law which has long been out of print.

quantities which are not divisible, and that such quantities are not confined to the class of what it has been customary to call intensive quantities. Some relations are quantities, and relations are not even conceivably divisible. For example, distance, the apartness of two points in space, is undoubtedly a quantity, but it is not when rightly regarded a divisible quantity. True, it is often mistaken for such, because it is confused with length (*Strecke*); but the thought of the length between two points in space is something quite different from the separation or distinction of two points in space. Distance is a relation, whereas a length is a whole containing parts. So again similarity and dissimilarity may be quantities. We talk about a greater or less degree of similarity, a greater or less degree of dissimilarity. But neither the one nor the other of these relations is a collection of units. Quite apart, then, from such intensive qualities as a pleasure or a pain, we are bound to admit indivisible quantities.

The next thing to notice is a further distinction which has an important bearing upon that between divisible and indivisible quantities. Although in ordinary speech unlikeness or dissimilarity (*Verschiedenheit*) is often used as synonymous with difference (*Unterschied*), yet on purely empirical grounds we are able to assert that when, as a result of comparison, we affirm or deny an unlikeness, we are not judging about difference. Unlikeness is asserted not alone of quantities; mathematical difference can only hold between quantities, and moreover only between divisible quantities. Thus, the difference between two lines is itself a line, but the unlikeness between two lines, like every other unlikeness, is a relation, and in no sense a length. Again, the difference may remain the same, whilst the unlikeness is not the same. Thus between 1 and 2 there is a much greater unlikeness than between 100 and 101, though the difference is the same. And in like manner, the unlikenesses may remain constant, whilst the differences differ,—a condition of things illustrated by Weber's law.

Now, all measurement rests upon the mental operation of comparison. As Mr Russell puts it, "without the immediate comparisons, which are necessary both logically and psychologically, nothing can be accomplished: we are always reduced in the last resort to the immediate judgment that our foot-rule has not greatly changed its size during measurement, and this judgment is prior to the results of physical science as to the extent to which bodies do actually change their sizes¹." And Meinong takes pains to make clear that to whatever extent physical

¹ *Principles of Mathematics*, 178-9.

operations may be substituted for mental, yet there is no possibility of basing measurement wholly upon the former. The process of superposing for example would have no meaning, did we not know that when one thing exactly "covers" another, the result for the most accurate comparison can only be equality. To look upon measurement as a purely physical operation would be tantamount to supposing that addition and multiplication had been converted into physical operations because both can be carried out by a reckoning machine.

Measurement, however, in the strict sense of the term, is applicable only to divisible quantities, in regard to which to say that *A* is double of *B* means that it is the magnitude of two quantities together, each of which has the magnitude of *B*. Such measurement may be either immediate or mediate. The former, which Meinong insists is applicable to time as well as to space, can be replaced by the latter when, as constantly happens, it is more convenient to measure directly a substitute for the object than the object itself. If, for example, it is a question of determining the length of a line which forms one side of a square, the problem may be solved by measuring any one of the other sides, if for any reason it is easier to do so. And the possibility of indirectly measuring indivisible quantities depends upon an extension of this method of substitution. In all such substitutive measurement (*surrogative Messung*) that which is actually measured is always a divisible quantity which serves as a substitute for the indivisible quantity. For example, distance, as we have seen, is a relation, and as such indivisible. But every distance, whether spatial or temporal, is associated with a length, and every length is associated with a distance. A distance may, therefore, be measured by measuring the length with which it is correlated. Similarly, in the case of the thermometer, only the height of the mercurial column can be, in the strict sense, measured, but we can take that to be a measurement of the temperature, so soon as an empirically determined regularity has been found to subsist between the height of the mercury and the states of temperature. So again, velocity is not identical with a length and the time in which it has been traversed, but we regard the velocity as measured when we have measured the length and time, and divided the former by the latter. The legitimacy of this process of substitutive measurement depends upon the extent to which there may accrue to it the advantages which are obtained from direct measurement. Three things, Meinong finds, give value to direct measurement. In the first place, a discrete term, namely a number, is substituted for an element of a continuum, and thereby the intractability

of the latter is relegated to the unit. In the second place, this number stands in the same relation of magnitude to other numbers as the given quantity stands to the other quantities of the same continuum. And, in the third place, the absolute limits, zero and infinity, which have validity for indivisible no less than for divisible quantities, coincide for the numbers and the quantities. Now, of the cases of indirect measurement to which reference has been made, those of distances and velocities participate in all three advantages, whilst to the measurement of temperature by the thermometer there accrues only the first of them. It appears, therefore, that some forms of indirect measurement are more imperfect or more rudimentary than others.

In the discussion of what he calls "psychical measurement," Meinong assumes that by sensual intensity is to be understood intensity not of the act of sensing but of the content, which he takes to be no less than the former psychical in character¹. He points out, what follows indeed at once from the prior investigation, that the distinction between psychical and physical does not coincide with the distinction between intensive and extensive. Some intensive quantities are to be met with in physical nature, whilst extensity, he thinks, is a characteristic of some psychical facts. Confining attention meanwhile to intensive psychical facts, Meinong dismisses as self-contradictory the conception, introduced by Fechner, of sensation-increments (*Empfindungszuwächse*). Because, however, there are not, and cannot be, units of sensation², it does not by any means follow that sensations are not measureable, any more than it follows that temperature is not measureable because there are no units of temperature. If regard be had to the changes of sensation-intensities, the problem does not present itself as in any sense a hopeless one. The thought of change rests upon the thought not of difference (*Unterschied*) but of unlikeness (*Verschiedenheit*)³, and the measurement of change carries us back to the measurement of distance. Change and distance do not, in themselves, imply increments and units. Whilst, therefore, the assertion that the change of sensation from S^1 to S^2 is equal to the change from S^3 to S^4 is a perfectly intelligible proposition, the assertion that

¹ I should differ from Meinong in this respect, but I am purposely avoiding that issue in the present discussion.

² Cf. Mr Bradley's article, "What do we mean by the Intensity of Psychical States?" in *Mind*, N.S. iv. 7. Mr Bradley contends that such units exist, although we are not able in fact to discriminate and fix them.

³ For such a phrase as *eben merklicher*, or *gleich merklicher Unterschied*, there ought to be substituted the phrase *eben merkliche*, or *gleich merkliche Verschiedenheit*.

S^1 is so many times greater than S^2 is not. If, then, the possibility of treating physical intensities as quantities be admitted—and in regard to some of them, at any rate, it cannot be disputed—the possibility of treating sensation-intensities in the same way as quantities must be conceded. In short, there is no theoretical difficulty in regard to the measurability of sense-contents. What difficulty there is is a practical difficulty, and arises from the circumstance that those operations which give to physical measurement its exactitude are not, as a rule, available. We are bound to have recourse to substitutive measurement, and the substitute must be a divisible quantity. If, however, there can be established on empirical grounds a definite series of correlations between changes of sensation, which are not numerically determinable, and changes of some extensive quantity, which are capable of numerical determination, then we should be just as entitled to take the magnitude of the latter as measuring the magnitude of the former, as we are entitled, for example, to measure temperature by means of the mercurial column of the thermometer. Only we must beware of taking for granted that no degrees of intensity are possible unless in fact we can measure them.

One of the chief features of interest in Meinong's analysis is the clear way in which it is shown that "psychical intensity" is not *sui generis*. Upon that assumption Bergson's argument throughout proceeds. His contention amounts, in short, to this,—that in order to be quantitative, a sensation would have to be built up, as Fechner supposed it was built up, of equal parts or increments. By bringing "psychical intensity" into line with intensity that is certainly not psychical, Meinong is enabled to free the former from an utterly incongruous conception. And when that is done, Bergson's thesis falls. It is perfectly true that the measurement of sensation-intensities is possible, if at all, only by a convention. But then that is equally true of the measurement of distances¹. It is perfectly true likewise that we are dependent upon the immediate apprehension of a change as revealed by the subjective comparison. But then all measurements depend in the long run upon immediate judgments of equality, and these, as also the immediate judgments of greater and less, are still

¹ The convention in the case of distances is, as Mr Russell states it, the following. It is agreed that, "when the distances $a_0a_1, a_1a_2 \dots a_{n-1}a_n$ are all equal and in the same sense, then a_0a_n is said to be n times each of the distances a_0a_1 etc., i.e. is to be measured by a number n times as great." This is a convention because "owing to the fact that distances are indivisible, no distance is really a sum of other distances." *Principles of Mathematics*, 180.

possible where measurement, in the strict sense, cannot be carried out¹. The real question is, how far the immediate comparison of sensations is reliable,—a question, no doubt, to which very varying answers will be given, but which does not affect the issue raised by Bergson.

§ 7. I confess I feel less satisfaction with Meinong's solution of the practical problem, acute and suggestive though his mode of handling it must be admitted to be. He maintains that, whilst in regard to sensation-intensities the appearance of equality can never be trusted, there can in normal cases be no question of an illusory appearance in the case of unlikeness. What to immediate apprehension appears unlike is unlike, although what is unlike only appears as unlike down to a certain limit—the threshold, namely—where the appearance of equality supervenes. The inferiority of judgments of equality as compared with those of unlikeness may lead to an apparent paradox, but it does so not only in the field of psychological but also in that of physical inquiry. And although at the disposal of the physicist there are vastly greater facilities for surmounting this defect of the faculty of comparison, it can never be completely overcome even by him. From this it follows at once that just appreciable unlikenesses need not be, as Exner, for example, assumed they must be, equal; but where there is equal sensitivity to unlikeness, there is a well-grounded presumption in favour of their equality. Upon these premisses Meinong rests the interpretation he has to offer of Weber's Law. So far as extensive sensations are concerned, it can be said, he thinks, that proportional sensations correspond to proportional stimuli and *vice versa*. With respect to intensive sensations, however, it is solely a question of equality of unlikenesses, and the law means that if R_1, R_2, R_3, R_4 be four stimuli and S_1, S_2, S_3, S_4 the corresponding sensations, then if the proportion $R_1 : R_2 = R_3 : R_4$ hold of the stimuli, the corresponding pairs of sensations exhibit equal unlikeness, and the unlikeness of S_1 and S_2 is equal to that of S_3 and S_4 . Owing to his confusing difference and unlikeness, Fechner assumed that just appreciable differences were themselves sensations, and his logarithmic formula calls, therefore, for unreserved rejection.

Far more uncertainty, however, attaches, I think, to Weber's Law than Meinong seems inclined to admit. The assumption that just appreciable unlikenesses can, even with his proviso, be regarded as equal, is destitute of any sufficient grounds. We know but very

¹ Cf. Russell, *ibid.* 182.

little of the conditions upon which the appreciation of minimal unlikenesses depends. One factor at least is hardly amenable to control,—the state, namely, of the adaptation of the end organs to impression. In the case of vision, for example, the conditions on which such adaptation depends are so numerous that they cannot be reduced to uniformity, and certainly cannot be eliminated. Such a fact as this alone would almost drive us to the conclusion that the minimal unlikeness is of variable nature. Moreover, these difficulties would recur in determining, as Meinong desiderates, where there is equal sensitivity to unlikeness. And then, again, the number of deviations from Weber's Law increases as investigation proceeds, so that it is fast becoming doubtful whether any field for its applicability will in the end remain. So far as cutaneous sensations are concerned, Rivers and Head find that it does not hold in respect of protopathic sensibility¹, and, indeed, it is hard to see how, except perhaps on Dr Myers's hypothesis, it is compatible with the 'all or none' principle. As regards taste and smell, the difficulties of obtaining experimental verification would certainly in any case be considerable, but, in spite of Miss Gamble's careful piece of investigation², it cannot, I think, be claimed that the applicability of the law to either of these senses has been placed beyond the reach of doubt. With respect to vision, there does not appear to be such independence of intensity and quality as would be requisite for the establishment of the law, and the same is to be said of hearing. At all events, the law cannot be taken to be more than an interesting empirical generalisation, based upon experiments that have not been purified from interfering circumstances, and must, even where it would seem to hold, be an expression for an extremely complex set of conditions.

§ 8. "It must never be forgotten," writes Sherrington, "that Weber's Law deals with judgments. The comparison of one sensation with a second of similar *quale*, but of dissimilar *quantum*, involves more than the mere neural process concerned with a simple sensation. From the very outset it works with ideas based on perceptions³." What is here said is certainly true so far as attention is confined to the elaborate comparisons on which Weber's Law is rested. But Sherrington's contention suggests an interesting question as to the ultimate psychological nature of the appreciation of unlikeness. The developed act of

¹ *Brain*, Nov. 1908, 428-9.

² *Amer. J. of Psychol.* 1898, x. "The Applicability of Weber's Law to Smell."

³ Schäfer, *Textbook of Physiology*, II. 932-3.

comparison always involves a reference to the objective order of fact as distinguished from the sense contents. It cannot, however, be supposed that any such objective reference is present in the rudimentary sense-experience out of which the recognition of an objective order has gradually emerged. That rudimentary experience could only have contained, at the most, the simple foundation on which the later process of judging psychologically depends. The inference seems inevitable that originally appreciation of unlikeness is itself a component of sense-experience. And the inference certainly gains confirmation from the consideration that the unlikenesses which we discriminate, be they great or small, are as much given as are, for example, the distinct sense contents which are pronounced unlike. Thus, I think, we are enabled to see that the sense-data we are supposed to compare in the developed act of judging degrees of intensity are not, in truth, sense experiences in the strict acceptance of the term. They are abstractions from sense-experience, and the isolation we artificially produce by working on the given material serves to disguise from us the actual nature of the experience we thus manipulate. Unlikeness, that is to say, is not something added to the contents of sense-experience from some other function of the mind; it does not arise for the first time when a complex act of judging comes into play; it is no less an element or aspect of that sense-experience than the distinguishable contents themselves. Such unlikeness may be of various kinds. The apprehension of quantity comes, we may agree with Mr Bradley, later than that of quality, if, that is, quality be taken at its crudest stage. But we have no ground for supposing that the elementary discrimination of either the one or the other necessitates a function of mind different in kind from that of sense-experience itself.

§ 9. If pressed to give a definite answer to the question whether intensity differences of sensation are quantitative, the reply, I presume, would have to be framed in some such terms as these. As the mathematician conceives of quantity, the only quantities whose differences may likewise be described as quantities are divisible quantities. Consequently, the difference or unlikeness of two intensive quantities is not itself a quantity,—which amounts, in other words, to saying that these quantities are not multiples of an element or unit similar in quality to themselves. Using for the moment the word difference in its non-mathematical sense, one would assert that just as the difference between two distances is not itself a distance, so the difference between two sensations is not itself a sensation. To quote Mr Russell's well-

known dictum, "the difference between two intensive quantities, in fact, differs from each as much as the difference between two horses differs from a horse." The distinction, however, which Mr Russell, in his *Principles of Mathematics*, draws between quantities and magnitudes would, I gather, enable us to speak of intensity differences of sensation as magnitudes. Magnitudes, as he would employ the term, are more abstract than quantities. A specific magnitude is a common property of a number of equal quantities. An actual foot-rule, for example, is a quantity; its length is a magnitude. A quantity is anything which is capable of quantitative equality to something else—that is to say, which is capable of possessing the same magnitude as something else. Properly, one quantity ought not to be described as greater or less than another, for the relations of greater and less hold between their magnitudes. On the other hand, properly one magnitude ought not to be described as equal to another magnitude, for the relation that would be really meant in such a case would be the relation of sameness or identity. Thus, for example, suppose a sound A possesses the loudness a and a sound B possesses the loudness β . A and B are each of them quantities; a and β are magnitudes. If A is louder than B , then the difference $a - \beta$, let us call it γ , is not a sound possessing magnitude; γ simply *is* a magnitude. If A resembles B in loudness, then a and β are not *two* magnitudes, a is the same magnitude as, or is identical with, β . And the difference or resemblance of A and B in loudness is a magnitude, because it is greater or less than other differences or resemblances, such, for instance, as the difference or resemblance in loudness of the sounds C and D . "Quantities not susceptible of numerical measurement can," says Mr Russell, "be arranged in a scale of greater and smaller magnitudes, and this is the only strictly quantitative achievement of even numerical measurement. We can know that one magnitude is greater than another, and that a third is intermediate between them; also, since the differences of magnitudes are always magnitudes, there is always (theoretically, at least) an answer to the question whether the difference of one pair of magnitudes is greater than, less than, or the same as the difference of another pair of the same kind. And such propositions, though to the mathematician they may appear approximate, are just as precise and definite as the propositions of Arithmetic¹."

¹ *Principles of Mathematics*, 183. Cf. 159.

ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE?¹ III.

By HENRY J. WATT.

1. *Which differences of sensation do we call intensive?*
2. (a) *What psychological place does intensity occupy amongst the attributes of sensation?*
(b) *In what relation does intensity stand to those modes of experience which bear a close psychological affinity to sensation and its attributes?*
3. *What is meant by the term 'quantitative'?*
4. *Is intensity a multitude or a magnitude?*
5. *What other objects besides intensity are at least magnitudes?*
6. *Can intensity possibly be treated as a multitude?*
7. *The source of the confusion.*

THIS question may be specialised into a series of questions. The answers given to them will not only indicate the special points at which differences of opinion may legitimately arise, but will also show that certain differences are due to a confusion of ideas and may therefore be eliminated.

1. Which differences of sensation do we call intensive? It is agreed, I think, by all that the classification of certain differences as intensive cannot possibly be called in question. The cutaneous, muscular, gustatory, olfactory, and auditory sensations all possess the undoubtedly similar attributes of intensity. We may, of course, enquire whether intensity is native to all these groups of sensations and, if not, how they came to acquire it. But that it is there, is surely not disputed. Nor does the absence of any marked degree of variation of intensity, as for example in the articular sensations, really present a difficulty. The only important problem in this connexion is whether the particular case of visual brightness is to be classified as a form of intensity or as a form of quality or the like. But we can afford to neglect this problem

¹ A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association in London, 7 June, 1913.

here and to confine our attention to the accepted cases of intensity. If visual brightness is to be considered intensive, the conclusions which are obtained for accepted forms of intensity will apply to it. But it is of interest to notice in passing that the proper classification of the attributes of sensation is not a perfectly simple task. There is room for serious divergence of views even at the present time¹. Introspection is, of course, the only ground upon which a true classification can be founded. But it must be granted that the first, or in fact, any single, deliverance of introspection about the inherent nature and connexions of experiences is not necessarily irreproachable. We have to learn to think truly about simple experiences, just as about the objects of the physical world we live in.

2. (a) What psychological place does intensity occupy amongst the attributes of sensation? In discussing whether intensity is quantitative or not, reference is frequently made to extensity, as if the latter were undoubtedly quantitative. A certain amount of prejudice against a negative judgment regarding intensity is thus created. If this prejudice is misleading, it must be removed. I do not think that extensity can legitimately be considered to be a variable attribute. It is invariable. It is not really less present in the sensation from a 'spot' than in that from an area; there is not more of it in a square inch than in a square centimetre of colour. Nor is a low tone properly more voluminous than a high one. What there is more of in these cases is extent or volume, not extensity or voluminosity. We have indeed said for long enough that low tones are more voluminous than high ones. We had perhaps good reason to fear a confusion between the volume of a tone and the volume of the physical material, if we had used the same term for both. But nowadays this confusion can hardly occur in reference to the study of sensation. It is no longer from without, but within the field of psychology that the danger appears.

The variant commonly referred to under the name of extensity, voluminosity, and massiveness, then, is not an attribute of sensation. It is a derivative, a higher product, a *Gestalt*, like that of a line or a curve, and it is variable in the sense of being greater or less, like these. The attribute of extensity² is the common basis of extensiveness, the real ground of fusion and continuity of sensation in the midst of differences of local sign and its analogues, position and pitch, which I prefer to group under the generic name of order³. If the same

¹ Cf. my discussion of pitch and other cases in this *Journal*, iv. 843 ff.

² I hope to deal with this attribute more fully at another time.

³ Cf. *op. cit.*

distinction is applied to the attribute of temporal extensity or duration, we obtain the following grouping of the attributes. Quality and intensity stand apart from the others, which fall into two pairs. Each pair comprises an extensive and an ordinal member and the two pairs may be named temporal and systemic. But, however tempting it may be for the purpose of systematic appearances, it is impossible to treat either quality or intensity as extensive or as ordinal in character. They are both ordinal in the sense of being self-disposing, but this peculiarity of them cannot be identified with ordinality; for upon differences in quality or in intensity none of those *Gestalten* or modes of sensory experience are founded which grow upon ordinal contents, viz. distance or interval, motion and others; and besides, quality and intensity are both more than merely self-disposing.

(b) In what relation does intensity stand to those modes of experience which bear a close psychological affinity to sensation and its attributes? These modes of experience have been forcing themselves with ever increasing insistence upon the notice of psychologists. There can be no doubt about their enormous variety and importance. Since the leading paper by Chr. v. Ehrenfels in 1890, by whom they were called *Gestaltqualitäten*, a large number of studies have been made of them¹. Such modes of experience are said to be founded upon contents, which may either be other modes or in many cases elementary sensations. I believe that there is always a certain amount of resemblance between the founded mode and its founding contents or some aspect or attribute of the latter, as well as an objective psychical dependence of the mode on its founding contents. These relations seem to me to form good ground upon which a body of pure psychological theory concerned with the interconnexions and development of experience may be built up². Many modes are variable and self-disposing, as being greater or less than one another in respect of their own peculiar phenomenal content. Distance and interval of time and motion are amongst the simplest of them, but there are many others³. The full and adequate study of these modes, their variety, relations, and theoretical explanation, is one of the newest forms of the psychological task, and will undoubtedly show itself to be one of its bulkiest parts.

In this connexion I see reason to differ from certain views indicated

¹ The first volume of a most valuable and important work by Karl Bühler on *Die Gestaltwahrnehmungen* has just been published (1913).

² Cf. my paper on the "Psychology of Visual Motion," in this *Journal*, vi.

³ This *Journal*, iv. 157 ff. For other modes cf. Bühler, *op. cit.*

by Dr Myers in I. § 2. The modes which stand next to elementary sensation seem to be, first distance and time-interval, and then, as a combination of these two, motion. Spatiality, if it is merely simple distance, seems to me to be in the matter of psychological origin independent of motion; if it is complex, such as is the spatiality of binocular vision, it does not seem to me to involve motion as a necessary psychological antecedent at all. Nor do I see any evidence for the existence of a psychological antecedent to intensity, simpler than intensity, from which intensity might arise by the integration of two or more of its varieties, as distance may be said to be integrated out of differences in the attribute of order. Any other speculations regarding the origin of intensity seem to me to be either inventions or to rest upon mistaken correlations.

3. What is to be meant by the term 'quantitative'? It seems to be agreed that there are two possible meanings. A quantitative object is either,

(a) A collective object, whether real or ideal—a number of material particles, persons, states of mind, events, or a number of ideal numbers, lengths, forces, universals. Let us call this kind of object a multitude.

Or (b) A self-disposing object, or an object say a_s , which in virtue of its own phenomenality disposes itself amongst other objects of the same group a_b, a_c, a_d , etc., in a definite manner, so that it falls between a_r and a_t , and not between a_d and a_f , and which in these relations appears to be greater than a_r and less than a_t . This kind of object is known as a magnitude.

4. Is intensity a multitude or a magnitude? [With regard to the expression 'intensity differences' in the title of this discussion, I take it to mean, in the first place, intensities, and only in the second place, if at all, differences of intensities, such as those between Ia and Ib , Ib and Ic .] On two points there seems to be agreement: (a) intensity is at least a magnitude; and (b) we cannot yet validly treat it as a multitude. We can, therefore, proceed to discuss the possibilities that are logically unaffected by these decisions. But before doing so it is well to turn aside for a moment and ask another question.

5. What other objects besides intensity are at least magnitudes? It is agreed, I think, that felt distance and motion and other such modes of experience or *Gestalten* are also at least magnitudes. We may, therefore, infer that the world of experience is rich in objects of this kind. Probably all forms of experience are, in some sense or to some degree, self-disposing objects. But a number of them cannot be considered to

be magnitudes, for example the above mentioned attributes of temporal and systemic order, percepts, recognition, concepts, thoughts and the like. The most obvious groups of experiential magnitudes are the modes and figures (*Gestalten*) of space and time, their combination in motions, and the various classes included under the term 'relations.' Magnitudes seem to occur by preference on what is obviously a duple or multiple foundation, such as we find in distance, succession, and change, or on what for various reasons may legitimately be held to be a duple or multiple foundation, as in minimal distances, motions, changes, etc. Feeling is one of the few cases in which a duple foundation seems to elude our grasp, but even here there is some sort of positive evidence¹. But there is at least no reason to doubt that differences of intensities are magnitudes and that we find it comparatively easy to arrange them and to observe and to indicate their apparent equality. In so far as we consistently maintain their phenomenal equality, we have as much reason to believe in the validity of our introspective judgments, as we have to believe in them in other regions of introspective work. But if *a*, *b*, and *c* are not multitudes, but experiential magnitudes, we cannot suppose that judgments regarding the equality of the differences between *a* and *b* and between *b* and *c* justify the statement that the difference between *a* and *c* is twice that between either of the former pairs. For the judgment regarding *a* and *c* has no bearing on the other two judgments, and *vice versa*. All just noticeable differences are equal in being just noticeable, but that does not make them equal increments. Nor can equal differences be considered to be equal parts of another difference, *i.e.* equal increments within the latter difference. Is there any sense in calling the tone interval *g—f'* twice as great as that between *g* and *c'*, because the intervals *g—c'* and *c'—f'* are equal in being fourths? Besides, a distance is not the difference between two points, but these and the stretch between them in a unity.

It would carry me too far from the object of this discussion, were I to enquire whether any non-mental, for example, material or ideal objects, are at least or solely magnitudes. Nor do I think it would throw any light upon the object of discussion.

6. Can intensity possibly be treated as a multitude? The conclusion I wish to plead for in this discussion is that it cannot, so long as the identity of the object under discussion, namely intensity, is maintained. I would suggest that an object cannot at one and the

¹ Cf. my discussion of it in this *Journal*, iv. 184 ff.

same time be directly immeasurable and indirectly measurable, as Meinong¹ declares and as Professor Dawes Hicks² agrees. Such a proposition can have an appearance of plausibility only by the substitution of a new measurable object for the one that is directly immeasurable. This substitution may be occasioned by the close connexion of the two objects in the world of reality, but it is none the less a substitution. To speak of a surrogative form of measurement is both misleading and wrong. What the medical thermometer measures is not the patient's sensations of warmth or cold or how warm or cold he feels. In this particular instance the departure from any sort of regular correlation between magnitude of felt warmth and degree of temperature is notorious. What the physician is usually concerned to know is the temperature of his patient's body. And that is as little a surrogative measurement of his patient's feelings as the sight or taste of the physic he offers is a surrogative cure for his patient's felt discomfort, however much or little the material physic may be suited to restore the patient's body to its normal condition. No one sets out to measure the sensed distances evoked by a thermometer scale, but only the lines or lengths of that scale. The latter are measurable, as are any multiple objects, in so far as they produce regular changes upon lines or lengths. In all cases it is only that aspect of the motion of matter which by an obvious convenience has come to be called temperature that is measurable. And similarly in other such examples.

I would also submit that in every case in which the treatment of single states of mind as multitudes is in any way made to be plausible, we find a substitution of objects of the kind mentioned. So for example in Fechner's formula, which is perfectly valid in so far as S in the expression $S = K \log I$ means 'the numerical value of S ,' if it exists. But unfortunately this value has no real object; the object and the value are purely imaginary. The fault here does not lie in the application of mathematical symbols and processes to the data of sense; for these are most certainly applicable to the data of sense whenever we have an opportunity of dealing with multitudes of these data, *e.g.* in the statistical manipulation of records of the frequency of visual and other images, in the study of memory and so on. The error committed by Fechner consists in applying mathematical symbols and processes to the data of sense without any proper psychological or objective justification.

¹ *Ztschr. f. Psychol.*, 1896, xi. 239.

² Cf. pp. 168 ff.

There is no theoretical difficulty in discovering truths that are non-truths. The difficulty is always a 'practical' one. The truths 'wanted' are simply not there to be had.

The substitution of objects I speak of may also be illustrated from Dr Myers's main thesis that the physiological correlate of intensity differences is a sub-group of extensive changes. That may very well be, but the thesis, as it stands, cannot be considered to afford any interpretation or elucidation of intensity or its differences. If it is a valid hypothesis, it certainly establishes a fact, it discovers a reality, a new kind of extensive distribution of physiological processes; and it sets this reality into relation with intensity. But that is all. We are not thereby brought any nearer to a treatment of intensity as a multitude. We merely know now a relation in which intensity stands that we did not know before. It does not affect the case in the least that the object with which intensity has been shown to stand in relation is itself a multitude. Physiology can be said to throw light upon psychological matters only in so far as a sufficient number of these relations between experiences and physiological processes are discovered to warrant the inductive assumption that certain known physiological units stand in certain relations to known psychical units or that certain as yet unknown psychical units exist and are related to these known physiological units in certain ways. I do not by any means deny the possibility of this inductive procedure. But I very much doubt whether the reverse does not constitute the method of greater illuminative power.

In short, no single state of mind can be treated as a multitude, not even the idea of 100 itself. Only the object of the idea of 100 can be so treated. But I do not mean hereby to imply that every object can be treated as a multitude. We must, of course, discover and determine whether any given object can be so treated or not. If we succeed, the object is a multitude; if we do not succeed, it may often still be a multitude. We cannot tell *a priori* where we are to look for objects that are multitudes and where not. Otherwise psychologists have made a sorry waste of their time and energy. It is quite possible that someone may yet prove by new methods that behind intensity there lie psychical objects now unknown to us which are to be considered as multitudes and are responsible for the phenomenon of intensity (cf. Myers, I. § 2). But not even such a proof would enable us to look upon intensities as themselves multitudes. Such a magnitude as intensity, like the so much discussed and practically useful distance, must remain a magnitude for ever and ever.

This may be enforced by another illustration. It is possible to maintain that felt distance is *realiter* psychologically founded upon repeated (i.e. a multitude of) sensational elements qualified by extensity and order and that thus differences of multitudes are the real basis of the differences of magnitude found in distances. But not even that would make distance in any sense a multitude. Only its real psychological basis would be a multitude¹.

If we had such as this imaginary knowledge of the real psychical basis of intensity, we might formulate the laws of mind and predict the psychical future better than we do now. But future mental states can be predicted by the knowledge of the physical world we already possess. We can, for example, arrange the illumination of a room so as to produce various mental effects. Yet that fact does not imply that we can measure intensity or its differences. Nor would the discovery I imagined.

If, finally, it be suggested that intensity can be treated as a multitude or measured by convention, I would submit that such 'measurement' is only a means of *naming* what stands in a real relation to something else that can properly be measured, as star brilliancies to the varying intensity of physical light.

7. The source of the confusion in these matters is an epistemological one—either a confusion of objects or a confusion of the immediate basis of knowledge in sensory experience with the objects of knowledge. In the latter case distance as sensed, for example, may be confused with length, felt motion or its velocity with motion through real space or the velocity of real motion. But it is surely absurd to suppose that any sort of reality—called velocity—exists that is a unitary magnitude in the sense in which colours and tones and felt velocity are such, and that nevertheless is measurable in numbers. Such a unitary reality is a myth, the hypostatization of a complex set of correlated relations in which a real or ideal object stands. Whether these relations are themselves real or ideal, actual or imaginary, makes, of course, no difference to the case.

If I rejoice in the possession of a new book, neither the possession nor the book thereby become feelings or emotions. If I know yonder tree is budding, neither the tree nor the budding thereby become either sensations, perceptions, or knowledge. They are only the objects of my knowledge and as such come into relation to my knowledge. So if I can

¹ Cf. the analogous theory given by E. R. Jaensch of the psychical representation of empty space, *Ztsch. f. Psychol., Erg.-bd. vi. 244 ff.*

measure lengths, why should I worry about not measuring distances as felt (*Gestalten*), when I have already ascertained that I cannot measure them? If lengths are in fact measurable, the equality or differences of distances may be the sensory basis on which the cognitive processes of conception and knowledge involved in the act of measurement build. But that is no reason why I should require or expect to be able to measure distances. If unitary distances are not to be converted into multitudes, we must just enquire how our cognitive processes can nevertheless make measurement of lengths possible. It is futile to think distances ought somehow to be measurable or to construe them so as to imagine them measurable. A real object has certain definite properties and it stands in certain definite relations to other objects; all one can do is to find out these things by knowing. Knowing powers will never by themselves alone change the properties of objects or set them into new relations, unless these be relations to my knowing or unless I somehow act upon the objects so as to change their real relations.

It seems necessary to make these remarks as there is a consensus of opinion that we actually do not succeed in measuring mental magnitudes such as intensity; and yet attempts are made to give the impression that after all our intellect is not so ineffective and useless as it is (most perversely) considered to be and that we really do measure these magnitudes; only we do not do the measuring in these cases directly or straightforwardly but indirectly or by substitution, or to put it bluntly by make-believe.

ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE?¹ IV.

BY WILLIAM BROWN.

- § 1. *Qualitative and Quantitative.*
- § 2. *The relation of physical measurement to extensity and protensity.*
- § 3. *Conditions fulfilled in physical measurement.*
- § 4. *The theories of Fechner and Delbœuf.*
- § 5. *The thermometer analogy.*
- § 6. *How far is a 'sense-distance' quantitative?*
- § 7. *Dr Myers's view.*
- § 8. *Professor Hicks's view.*
- § 9. *Dr Watt's view.*

IN view of the length and thoroughness of the foregoing discussion on this subject, I trust I may be excused for making my remarks as brief as possible. After setting out my own reasoned opinion on the matter, in the fewest possible words, I will indicate how far I am in agreement with my predecessors in the argument, and discuss those points where I am in dissent.

§ 1. One of the most general statements that can be made about the stream of consciousness is that every moment or pulse of this stream is qualitatively different from every other. Any concrete datum of experience is only identical with itself. To say that it is equal, greater or less, as such, than any other would be meaningless. Nevertheless the qualitative similarities observable within experience justify the distinction of aspects of consciousness each showing a homogeneity, or a unity in difference, of a particular kind. Examples of such aspects or attributes which are relevant to our problem are extensity, protensity, clearness, penetratingness (*Eindringlichkeit*), saturation (of a colour), brightness (of a colour), and intensity. They are not only homogeneous but also show degrees and admit of the use of the words 'greater' and 'less' in their description. They may therefore be regarded as

¹ A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, in London, 7 June, 1913.

magnitudes. Whether they are to be regarded as quantities, or measurable magnitudes, is another question. A consideration of the cases of extensity and protensity makes this more than doubtful. These aspects of consciousness must clearly have been the preconditions of the development, in the course of mental evolution, of our knowledge of objective and quantitative space and time, since if we think them away such knowledge would be inconceivable. Without attempting the difficult task of showing how this development took place, we may at least take it as a fact that the use of the principle of superposition and the discovery of fixed numerical relations, in terms of definite units, between things in space and time gave birth to physical measurement. Measurement was produced by measuring and not by philosophical analysis. Quantitative relations are characteristics of the real world which are proved to exist by the tentative process of experimenting. Whether forms of measurement other than the physical are possible can only be decided in the same way.

§ 2. But this very success of physical measurement might tend to suggest that attempts at psychical measurement rest on a misconception. The differences of degree of extensity and protensity have found their real measurement in the measures of surface extent and time. And although the existence of illusions shows that this correlation of psychical feeling and physical size is not a complete one, the explanations of these illusions which are asked for and are found rest on belief in a real correspondence. All that follows, however, from such an argument is that the accurate measurement of extensity and protensity is not worth the trouble of carrying out, not that it is theoretically impossible.

§ 3. For measurement to be as complete as that in physical science, we have seen that the following conditions must be fulfilled: (1) the thing or attribute measured must be homogeneous and show degrees of more and less, (2) a unit must be discoverable, of which the given magnitude may be said to be a certain multiple or sub-multiple, (3) there must be a zero from which the measurements are made, (4) it must be possible, theoretically at least, to superpose one magnitude on another, and so get the measure of a difference or the proof of an identity. It does not, however, necessarily follow that in cases where some of these conditions are not fulfilled the thing or attribute is not a quantity. There may be different kinds of measurement, of different degrees of completeness. Experience alone will tell us how complete it may be made.

§ 4. Fechner tried, by building on Weber's Law and making a psychological use of the concept of the limen, to get a scheme of measurement of sensation-intensities themselves. He failed, because no sensation-intensity can be regarded as a sum of smaller intensities. Intensities, like the other general attributes which we have mentioned, obey only the first, and, perhaps, the third of our four conditions. Delbœuf quickly showed, however, that the sensible contrasts, or sense-distances as they may be called, between sensation-intensities do admit of division and summation. This conclusion was not based upon a *a priori* argument but upon experiment.

He showed that the problem of finding an intensity of grey which should lie half-way between two more extreme shades of grey, or bisect the sense-distance (*contraste sensible*) between them, was a real one and admitted of a real solution. Despite a certain variability of the judgment in the case of different persons or of the same person at different times, which was only to be expected from the nature of the experiment, there was an average constancy of the result which indicated with a high degree of probability that the problem had been genuinely solved. If it be objected, with Bergson, that the subject of the experiment judges the intensities in terms of the corresponding stimulus-values with which he has become acquainted in previous experience, we may point to the fact that the stimulus-value of the middle grey is the geometric, and not the arithmetic, mean of those of the extreme greys. This experiment has been done many times in my laboratory by numerous students during the last few years and the results have always been definite and unequivocal in nature.

Whether it be light-intensities or sound-intensities or the intensities of sensations aroused in lifting weights, the results show a remarkable constancy. Stimulus-values so chosen as to form a geometrical progression give sensation-intensities forming a series of equal-appearing intervals. Ebbinghaus expresses this relation thus:

$$\overline{SS}_0 = k \log \frac{R}{R_0},$$

where \overline{SS}_0 is the sense-distance between S_0 , the conventional zero-sensation which may itself be any degree of intensity and not actually zero, and a given sensation S , R_0 , R are the corresponding stimulus-values, and k is a constant. The correspondence of the equation with that expressing the Weber-Fechner Law suggests that the just noticeable differences of sensation-intensity involved in the latter may not only be regarded as minimal sense-distances but also as equal to one another in

different parts of the scale of intensities. The equality of just noticeable differences (distances) of sensation, which Fechner assumed on the basis of introspection, may be experimentally tested, by noting the number of just noticeable differences necessary to take the observer from one end to the other of a number of equal-appearing sense intervals. The results of these experiments are somewhat conflicting, but the balance of evidence seems in favour of the equality of the just noticeable differences.

§ 5. In the choice of a conventional zero from which sense-distances may be measured, the analogy of the thermometer is often referred to as a justification. Just as two arbitrary points, corresponding to the freezing point and boiling point of water respectively, are chosen and the distance on the thermometer tube between these two points is divided into a hundred equal divisions, so a sensation of low intensity (not zero) may be taken as the conventional zero, another sensation-intensity near the upper limit of intensities may be arbitrarily fixed as 100, and the sense-distance between them may then be divided into 100 equal sense-distances. In such a case as this the intensity bisecting the two extremes would have the value 50, as being 50 units of sense-distance removed from the conventional zero. It seems to me that Dr Watt has mistaken Prof. Dawes Hicks and Meinong in regard to this analogy, by assuming that Meinong took it as more than an analogy and considered the thermometer as an instrument for the indirect measure of subjective intensities of heat-sensation. Meinong saw in it a means of indirectly measuring temperature, which is a very different thing, and for this reason Professor Hicks's use of the argument seems to me to be perfectly sound.

§ 6. The system of psychical measurement which I have outlined above is that accepted by Stumpf, Ebbinghaus, and Titchener among modern psychologists. It fulfils the first three of the four conditions found satisfied in physical measurement (see § 3); but although the principle of superposition is inapplicable, this defect is not sufficiently serious to rob it of the claim to be called measurement. Although it cannot, for this reason, hope for so successful a career as that which physical measurement has enjoyed, it nevertheless has many possibilities before it which when realised may transform the science of introspective psychology. To object that sense-distances can never be equal because they start from different degrees of intensity is to overlook the element of abstraction essential to all measurement. And what the system lacks in the matter of superposition is in some degree made up to it in the

possibility of controlling and measuring the corresponding stimulus-values. By means of the logarithmic law we can pass from the one to the other at will.

§ 7. Turning to Dr Myers's treatment of the problem I find nothing that is in conflict with the theory just mapped out, but much that is a very valuable supplementation of it. His theory of the physiological correlate of sensation-intensity is especially valuable as emphasizing the differences in the laws of intensity-change obeyed by the different classes of sensation and providing a physiological explanation of them. There is some little difficulty in imagining a complexity of efferent tendencies and motor responses sufficient to account for all the shades of qualitative difference between the elements of sensory consciousness and in some parts of Dr Myers's essay there are phrases which seem to imply interactionism rather than the psycho-physiological parallelism which is the basis of his general system (*e.g.* "At bottom, differences in type of movement must be the cause of differentiation in the quality of sensation; it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable in promoting different types of response"); but apart from these small points, which are not real objections, I can accept his views. On the psychological side, I should agree with him that sensation-intensity is *sui generis*, but this does not prevent contrasts of intensity being quantitative and therefore measurable.

§ 8. With Professor Dawes Hicks on the psychological aspect I am equally in agreement. He has gone into the epistemological side of the problem so thoroughly that it did not seem necessary for me to devote any further attention to it. To my own mind, the question now is one of practice; not "Is measurement of intensity differences theoretically possible?" but "Has such measurement been practically achieved?" and the answer to this question seems to be in the affirmative. It is important to have so convincing and detailed a refutation of Bergson's negative as Professor Hicks gives, and also to know that Meinong has made mental measurement so theoretically plausible; but as soon as we can get together a body of practical results of which a beginning (a very small one, it is true) has already been made, these questions will become of merely academic interest, although of course their importance for metaphysics will remain.

§ 9. I find Dr Watt's paper a little disappointing, since, although he says much that is of interest about intensity in its relation to other attributes of sensation and other forms of consciousness, he seems to

think that by denying to intensity the characteristic of being a 'multitude' he has settled the question of 'quantity.' We have seen above that 'sense-distances' are quantities even although intensities themselves are not. Moreover his criticism of Meinong's surrogative form of measurement seems to rest on a misunderstanding, as I have already pointed out. Finally I find it impossible to agree with him when he writes, "I do not think that extensity can legitimately be considered to be a variable attribute. It is invariable. It is not really less present in the sensation from a 'spot' than in that from an area; there is not more of it in a square inch than in a square centimetre of colour." It seems to me that such a view would deprive extensity of all its value in enabling us to understand the development of the perception of surface extent, besides being unsupported by introspection. In extensity distinctions of 'more' and 'less' seem to be as clearly present as in intensity or any other of the attributes of sensation, and to this extent extensity, like intensity, is a magnitude.

THE AESTHETIC APPRECIATION OF MUSICAL INTERVALS AMONG SCHOOL CHILDREN AND ADULTS.

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- I. The purpose of the experiments.*
- II. The method of experiment with adults.*
- III. The order of popularity of the intervals.*
- IV. The aesthetic effect of the different intervals. Major and
minor intervals. The octave. Concords felt as discords.*
- V. The method of experiment with school children.*
- VI. Results of the experiments with Elementary School children.*
- VII. Results of the experiments with Preparatory School children.*
- VIII. Comparison of the results of the experiments in the
Elementary and Preparatory Schools.*
- IX. Tests for a 'musical ear.'*
- X. Introspection of school children.*
- XI. Sex differences in the Elementary School experiments.*
- XII. Summary of results and conclusions.*

I. The purpose of the experiments.

IN 1910 some experiments were begun in order to test the aesthetic appreciation of musical intervals among school children. The object was to discover, if possible, something as to the development with age of a feeling for consonance, and to determine the differences in this respect among children belonging to different cultural groups and having had different degrees of musical training. It seemed desirable also to obtain results from adults, for the sake of comparison.

Apart from this, I wished to ascertain the extent to which individuals could be divided into 'perceptive types' according to their attitude towards musical elements, as Mr E. Bullough has classified

them in reference to colours¹, and to note any marked difference between the sexes in reference to their appreciation of musical intervals².

Further, a great deal of uncertainty still exists as to the order of pleasingness of the twelve intervals playable upon the piano within one octave, though there is of course a general agreement as to which intervals are dissonant and which consonant³. Some indeed seem to identify pleasingness with consonance. Others have doubted whether we can isolate an interval so as really to hear it alone, their view being that the pleasingness of an interval will depend not merely upon its degree of consonance, but also on the more or less vague suggestions of other notes. This is of course directly opposed to the assertion that one can isolate an interval and that all consonances are then more pleasing than any dissonances. As to the varying degrees of pleasingness of the intervals one must expect great differences among individuals, though there seems to be a general agreement that to the modern ear the Third is the most pleasing interval, whilst during the middle ages the Fifth was probably the most popular, and with the Greeks, the Octave.

We shall describe the experiments with adults first. The method of experiment with the children was substantially the same.

II. *The method of experiment with adults.*

The subjects of these experiments were university students at St Andrews, or students in the Training College, Dundee. They numbered 146, of whom 84 were women and 62 men. The great majority of the women were Scots: of the men about one quarter were English, one fifth Welsh and the rest Scots. Most of the tests were done with a Chappell piano in excellent condition, the rest upon a new Bechstein, both regularly tuned⁴. The subjects were taken in groups—about 18 in each group on an average. The necessity of perfect silence and absolute independence of judgment was emphasized. All the subjects had undergone a course of experimental psychology and, I think, must have appreciated the importance of guarding against

¹ This *Journal*, II, 406.

² The discoveries as to perceptive types are not given in this paper. They will be included in a subsequent joint paper by Dr C. S. Myers and the present writer.

³ As to the degree of consonance of the various intervals see C. S. Myers's *Text Book of Experimental Psychology*, 2nd edition, I, 27.

⁴ It should be recalled that, owing to the tuning of pianos by the method of 'equal temperament,' the intervals (with the exception of the octave) have not their exact theoretic value.

the slightest amount of suggestion in such experiments. They were provided with paper on which they were asked to record their judgments upon the chords played, stating whether they found them very pleasing, pleasing, slightly pleasing, indifferent, slightly displeasing, displeasing, or very displeasing, adding the reason why, if possible. Each interval was played twice, the notes first being held down for three seconds. Then followed an interval of three seconds' silence, whereupon the notes were again struck and held for three seconds, the periods being timed by a stop watch kept going continuously.

I regret that I had no mechanical means of insuring that all the intervals should be struck with uniform force. But I may add that, though by no means an expert musician, I have been informed by professional musical critics that my touch is accurate and sensitive, having been trained from early boyhood, and judging from the introspective remarks of all the subjects and the answers to specific questions of mine addressed to some taken individually, I believe that I was able to avoid giving any appreciable emphasis to any one note in any of the intervals, and to preserve a fair uniformity in the loudness of the intervals¹.

In some preliminary experiments I used the notes *cd^b*, *cd*, *ce*, etc. up to the octave *c'c''*. But as certain observations of subjects revealed preferences based on the pitch of the intervals, I decided that a better arrangement of notes was possible. I was led to distrust the assertion sometimes made that the pitch of a combination of tones is approximately that of the lower of its constituents. One feels diffident in differing from so expert an observer as Stumpf², but my own introspection, and that of several other individuals specially tested, suggests that the pitch of the *higher* note in any combination near the centre of the piano is likely to be an influential determinant of the apparent pitch of the combination. I gave to several subjects the test suggested in this connexion by Stumpf. They were asked whether *c⁰c'* or *c'c''* differed the more from *c'*. Stumpf asserts that the lower octave differs more, thus confirming, he says, his assertion that *c⁰* gives the pitch of *c'c'*. I put this test to twelve subjects, and nine of them asserted most emphatically the opposite to what Stumpf says. Eleven of the twelve

¹ Even if one note is slightly louder than another in an interval, it does not, according to Stumpf, affect the consonance of the interval (*Beitr. zur Akustik und Musikwissenschaft*, II. 10, quoted in Lalo's *Esquisse d'une Aesthétique Musicale*). But of course there might be some difference in the feeling effect of the interval, even though the fusion were not changed.

² *Tonpsychologie*, 2te Aufl. II. 384.

also asserted that g^0g' appears higher in pitch than b^0d^\sharp , though the fundamental of the former is two tones lower than that of the latter. This suggests that the pitch of an interval appears (for most people) to be nearer that of the higher of its component notes rather than the lower. It seems to me possible that owing to his exceptional musical capacity and training there is a much stronger tendency for Stumpf to refer every interval to its fundamental tone than is the case with the average individual, thus bringing the fundamental into greater prominence in the field of attention. Hence its greater influence in determining the apparent pitch of the interval¹.

Had I performed all the above tests before conducting my experiments I might have felt inclined to keep the upper tones of all the intervals always the same, or I might at least have let them vary in pitch less than the lower tones. As it was, I resolved upon a compromise, choosing the following series of notes to represent the various intervals,

minor second,	$c'db$,
major second,	$c'd'$,
minor third,	b^0d' ,
major third,	b^0d^\sharp ,
fourth,	bb^0eb ,

and so on, alternately raising the upper note or lowering the lower a semitone until for the octave we get g^0g' . Thus the mean between the two notes in any interval would always be c' or c^\sharp , or some note between these.

It was for the sake of the children in the main that this arrangement was made, for it was surmised, rightly as it proved, that they would be much more influenced by absolute pitch than would the adults. That the new arrangement was justified is, I think, shown by the following fact. As in the case of the adults, the children were asked to give their reasons for liking or disliking an interval, and a fairly frequent reason was that the notes were 'nice and high,' or 'too low.' Now it appears that such votes given by the children for an interval "because it is nice and high (or low)" are fairly equally scattered over the various chords, and that judgments against intervals because they are high (or low) are also similarly scattered. The evidence indeed (in

¹ Two of the three exceptions who agreed with Stumpf with regard to the first test were highly trained musicians. With one of these subjects the nature of the intervals compared seemed to have some influence upon the apparent pitch. Thus $c'g'$ was judged higher than $d'f'$, but g^0f' was judged lower than b^0d^\sharp .

so far as the reasons given by such young children can be trusted) tends to support the view I have put forward that the higher note is more influential than the lower in determining the apparent pitch of an interval. Thus the following table, giving the judgments of children from eight years to thirteen inclusive, shows that the third $bd\sharp$ is judged to be 'low' more often than the octave $g^o g'$, and 'high' less often than the octave, presumably because the upper note of the third is lower than the upper note of the octave.

TABLE I.

Octave, $g^o g'$.			Major third, $b^o d\sharp$.		
Number of times spoken of as			Number of times spoken of as		
	High	Low		High	Low
Age 13.....	0.....	3	Age 13.....	1.....	1
„ 12.....	1.....	1	„ 12.....	0.....	4
„ 11.....	3.....	2	„ 11.....	2.....	6
„ 10.....	5.....	2	„ 10.....	3.....	2
„ 9.....	2.....	1	„ 9.....	0.....	1
„ 8.....	0.....	2	„ 8.....	2.....	1
Totals	11	11	Totals	8	15

Before each sitting the intervals were arranged in a haphazard order, care only being taken that an interval should not appear often in the same position, and also that the same succession of two pairs should not recur. The twelve intervals were then played in the order arranged, time being given after each for the subjects to write their introspective remarks at once. After the twelve had been given, some easy tests of musical capacity such as have been used by Stumpf were given¹, and then the twelve intervals were played over again in the reverse order to the first, in order to distribute equally among the intervals any effects due to familiarity, etc., and to equalise as far as possible the effects of contrast due to the chord preceding the one played. As they were thus arranged in about twenty different orders for the adults, probably the various effects of contrast were fairly scattered. The likelihood of such disturbing effects was further lessened

¹ The subjects had to say whether one or two notes were being played on the piano, and which of two successive notes, separated by a tone or semi-tone, was the higher. This proved so easy for most of the adults that I did not attempt any division of the students on the basis of the results. Only about half a dozen students could be reckoned as 'unmusical' upon the basis of the tests. The same tests were performed on the children; for the results see page 210.

by the long interval allowed for the writing of introspective remarks. But observations made by some subjects show that one cannot hope to get rid of them entirely.

III. *The order of popularity of the intervals.*

On the basis of the judgments expressed by the subjects, the intervals can be arranged in order of popularity. In reckoning the scores of the various intervals the following values were assigned. For 'very pleasing' 2, 'pleasing' 1, 'slightly pleasing' $\frac{1}{2}$, 'indifferent' 0, 'slightly displeasing' $-\frac{1}{2}$, 'displeasing' -1, 'very displeasing' -2. This scale gives the following results for all the 146 adult subjects.

TABLE II.

Major third	324	Tritone	153
Minor third	261	Fifth	139 $\frac{1}{2}$
Octave	246 $\frac{1}{2}$	Major second... ..	- 99
Major sixth	243	Minor seventh	- 162
Minor Sixth	214	Major seventh	- 316
Fourth	157 $\frac{1}{2}$	Minor second	- 368

Of course we cannot assume from this list that the major third was the most pleasing and the minor second the least pleasing of the intervals to all subjects. In some cases it was obviously not so. We can only say that *on the average* the major third is the most pleasing¹.

As will readily be seen, the order is very far from that of degree of consonance. The major third scores much more highly than the octave, both the thirds and both the sixths score higher than the more consonant fourth and fifth, and even the tritone—which has been reckoned on the border line between consonants and dissonants, is found more pleasing on the average than the fifth—the most consonant of all the intervals except the octave. The degree of consonance then is by no means coincident with the degree of pleasingness.

If the votes of men and women are separated we get the following results:

¹ Theoretically it is of course possible that an interval—say the fourth—owes its intermediate position merely to the fact that some subjects like it most and others dislike it most. As a matter of fact no interval showed such results. A decreasing number of 'very pleasing' judgments goes with a decreasing number of 'pleasing' and an increasing number of 'indifferent' and of 'displeasing' judgments.

TABLE III.

MEN (62 subjects)				WOMEN (84 subjects)			
Major third	141½ (187)	Major third	183½
Octave	118½ (148)	Minor third	156½
Major sixth	105 (140)	Major sixth	138
Minor third	104½ (139½)	Octave	128
Minor sixth	103½ (138)	Minor sixth	110½
Tritone	67½ (91)	Fourth	93½
Fourth	64 (85)	Tritone	86
Fifth	61½ (83)	Fifth	78
Major second	-41 (-55)	Major second	-58
Minor seventh	-67½ (-90)	Minor seventh	-95
Major seventh	-120½ (-160)	Major seventh	-196
Minor second	-152 (-202)	Minor second	-216½

The numbers in brackets represent the votes of the men increased proportionately to make them comparable with those of the women.

In view of the great variations in the preferences of different individuals, there is no very striking difference between these two orders with the exception of the greater popularity of the octave and the minor sixth among the men, and of the minor third among the women.

IV. *The aesthetic effect of the different intervals.*

The introspective remarks throw some light upon the nature of the effects produced by the various intervals. We will consider them in order of their popularity.

Major third. A great variety of reasons are given for liking the major third. It is described as harmonious, melodious (frequent), something like the previous chord (which was an octave and was liked), well balanced, blending, mellow, soothing (frequent), calm, sad (frequent), solemn, minor, feeling of anticipation, melancholy, firmness tinged with pleading, strong. Associations with the major third are solemn music, church bell, Dead March in Saul (frequent), Amen in church. Reasons for disliking the major third slightly, or finding it indifferent, are the following: unfinished, feeling of lethargy, slightly too solemn, Dead March in Saul suggested. The major third was disliked only four times, in each case by a woman and either because it was too sad, or meaningless.

Minor third. Very varied reasons are given for liking the interval. Comments include the following: soothing, mournful, solemn, suggests Dead March, refined, cheerful, dreamy.

It is only disliked seven times, the great drop in the score (compared with the major third) being due to the comparative frequency of the judgment 'indifferent.'

Major and minor intervals. The question has been discussed as to whether the effects of major and minor keys are 'inherent' in the intervals themselves, *i.e.* whether the major chord strikes some essentially cheerful, responsive note in our nature, the minor rousing equally 'naturally' a sad feeling; or whether the different effects of the two keys are due merely to association. If the latter we must suppose that the custom of setting sad songs to minor keys originated without any felt suitability of the key to the ideas, but that gradually, by repetition of the association, we have come to connect the two, so that a piece of music in a minor key now appears to us sad or plaintive. In favour of the latter view we have the fact that in some civilised countries the major key is frequently used for sad songs and the minor sometimes for quite cheerful or even merry ones. Thus we find dance music and even comic songs set to a minor key. Further, it is asserted that the music in the minor key played by some primitive peoples, while sounding sad and dirge-like to us, does not appear to be so to the natives¹.

The results of the present series of experiments, as summarised in the following table, certainly suggest that there is, inherently or through association, no more sadness in the minor third or minor sixth than in the major third or major sixth.

TABLE IV. *Number of times interval is described as sad or plaintive.*

	Major third	Minor third	Major sixth	Minor sixth
Men	10	5	16	7
Women	16	6	11	7
Totals	26	11	27	14

The figures show that the major intervals are described as sad or plaintive twice as often as the minor. Of course we must remember that we are only testing the effect of one interval, and that, too, with the notes played simultaneously, whereas in a piece of music in the minor key we should also have the intervals given by consecutive notes. Further, their relation to the scale as a whole is brought out more fully in a piece of music and this is doubtless the most important point in determining the impression made by the music. The recognition of the key as minor however is not necessary for the effects of sadness and

¹ Cf. Müller-Freienfels, *Psychologie der Kunst*, II. 70.

plaintiveness, for these may be felt by persons who are quite ignorant of the distinction between major and minor keys. It is noteworthy that five times the major third was actually described as minor while the minor was never called 'minor.' Probably this particular major interval was felt to be 'sad' and was termed 'minor' because of the familiar association of the two in music.

Even when a third note is added in these experiments and the chords *ceg* and *ce^bg* are played, the major chord is still termed sad as frequently as the minor, though judging from his own introspection, the present writer is greatly surprised at this result. Thus among about forty adults to whom these chords were played (among twenty other chords), eight persons described the major chord and six persons the minor chord as sad¹.

The evidence, then, of these experiments is that the minor intervals (when the notes are played simultaneously) are not felt as sad even to the same extent as the major intervals. And this is in favour of the view that the general significance of the minor key for modern European ears is not due to an effect inherent in the relation of the notes in a minor interval, but is more probably the effect of association. Further, for the average person it seems that more is necessary as a basis for this association than isolated minor intervals or chords.

The octave. The octave is often termed indifferent and is disliked more frequently than the minor third. It is found by some too tame, dull, thin and skimpy, lacking in meaning, whilst others are attracted by its clearness, smoothness, idea of no hindrances, or describe it as bold, bright, strong and cheerful, giving the feeling of rest.

We saw that the octave was considerably more pleasing to the men than to the women, and we find that only one man among 62 judged it positively displeasing, while 15 women among 84 do so. The introspective remarks give us a clue to an interesting sex difference

¹ In these trichord (*Dreiklänge*) experiments the chords had always *c'* as the tonic. This fact precludes an explanation that might conceivably be put forward as to why the major is described as sad, or even as minor, in the interval experiments, viz. that sometimes the influence of the preceding interval might determine that a major interval should appear as part of a minor scale. For example, when *a^bg^b* is followed by *c^be^b*, if the tonic *a^b* were still held in mind when *c^be^b* was played (which would be after an interval of 2 or 3 minutes) and if *c^be^b* were then heard as in the key of *a^b*, the *c^b* might give, with the retained impression of the *a^b*, the impression of the minor third of the scale *a^b*. This seems to me extremely unlikely in view of the long time between the playing of the intervals and also in view of the fact that very few of my subjects were even average musicians. Further the major third usually followed intervals which could not produce such an effect. In any case, the experiments with the trichords were free from even this remote possibility.

here. Whereas the men frequently like it because it is strong, firm, bold, suggesting majesty and force, this aspect of the octave does not appeal in the same way to the women; it is even displeasing to some of them, who speak of it as too assertive, hard, or harsh.

The major and minor sixths. Coming to the next intervals in order of preference, one is immediately struck with the enormous individual differences which now show themselves. Thus of four persons listening to the major sixth at the same time, one speaks of it as 'soothing,' the second as 'rousing,' the third as 'sentimental,' and the fourth even as 'jarring to the ear.' This is generally characteristic also of the fourth, tritone and fifth. A distinct tendency however is noticeable for the sixths to be felt as sad or solemn. Here for the first time we find suggestions of disharmony, half a dozen or more subjects finding that the notes do not blend satisfactorily, a remark which applies also to the fourth, fifth and tritone.

Fourth and fifth. The low position of these intervals is not traceable, on the basis of the introspective remarks, to any definite disagreeableness in them. The average value of the votes, it will be seen, yields for each of them nearly 0.5, the equivalent of 'slightly pleasing.' But they rarely become 'very pleasing' and are often judged 'indifferent,' sometimes with the description 'ordinary,' 'no impression.'

It is very remarkable that these—the three most consonant intervals after the octave—are sometimes spoken of as discords, or as lacking in harmony, oftener indeed than the tritone¹. Possibly we have here a suggestion that the conventional concord may come to appear less consonant by becoming for some reason very unpleasant (perhaps from appearing, first commonplace, and then monotonous). On the other hand there is ample introspective evidence that dissonant intervals, where pleasing, are sometimes felt as consonant. This is probably the case when they are introduced appropriately in musical compositions. But the above statement is not confined to the cases where the discords are heard as leading to a pleasant resolution. The impression occurs too when they are heard alone.

As to the *four discords* we may further remark that any one of them may appear pleasing through some definite association or symbolic suggestion, and that sometimes discords are liked as a pleasant change from a preceding harmonious interval.

¹ It is possible that some of these judgments may be due to the fact that the fourth and fifth are somewhat out of tune as sounded on the piano. But at least one subject who repeatedly gave such judgments was distinctly weak in detecting dissonances.

V. The method of experiment with school children.

These experiments were performed on one hundred and ninety-five boys and girls from two Elementary Schools in Dundee, between the ages of six and fourteen, and upon seventy-six girls between the same ages in a high class Preparatory School in St Andrews¹.

The object, as already stated, was to discover if possible something as to the development with age of a feeling for consonance, and the difference in this respect between children belonging to different cultural groups and differing with respect to musical training. There are, of course, enormous individual differences even among children as regards the sensitivity to music. By means of these tests one could only hope to study the averages of a large number of children groups of which differed in culture and age.

The method of procedure was the same as in the experiments with adults, with the following exceptions. I played all the intervals over to the children before the judgments on each interval were given, in order to show them the kind of test they were to expect. During the latter half of the experiments I also added a new interval (ninth) with which I always began the list, though the judgments on it were ignored.

In the case of the Elementary School children I had the cooperation of some of my own students, partly because many of the children were too young to be trusted to make satisfactory written records, and partly in order that the students might have some experience of research work. The Elementary School children were taken in twelve groups of about fifteen pupils, of various ages, at a time. Thus the intervals would be played in twenty-four different orders in the course of the experiments, a new arrangement being made for each group, and the exact reverse of it also being used at each sitting.

The children were distributed over a large room, and a child was allotted to each student, who recorded the child's judgments. All the students had had some training in experimental psychology. The method and purpose of the experiments were carefully explained to them beforehand. I impressed upon them especially the supreme importance of giving no sign of approval or disapproval of the child's judgments, and of avoiding any possible suggestion. At each test I explained the experiment to the children somewhat as follows. "I am going to ask you to listen carefully while I play some notes on the

¹ In a few cases the same pupil returned for the test again a year or so later. In these cases they are reckoned twice in counting the number of children.

piano, and then to say whether you like them or not. If you can, say *why* you like them or don't like them. I want you to say exactly what you think. No one will see your answers except myself. You are not in school now, and no one will blame you for what you say." A little friendly talk soon seemed to put the children at their ease. The students were told to say nothing that was not absolutely necessary—which practically reduced itself to asking 'why' of those children who seemed to need encouragement to state their reasons. As I had all the students in full view I was able to see that this rule was remembered. We know from our experiments on the adults the average order of preference for the intervals with the students. Thus if there were any influence of suggestion on the part of the students we know in which direction it is likely to work. Personally I believe that in these experiments such influences were extremely small.

Such collective experiments have grave dangers and disadvantages unless very carefully carried out; but apart from the fact that they enable one to examine far more subjects than would otherwise be possible, they may, I think, have two advantages. In the first place the children seem more at their ease when a large group of them is examined simultaneously than when only one child is tested at a time. Further, each group of children included children of various ages from six to thirteen, and both boys and girls. Thus if any irregularity did occur (as for example if an interval were played somewhat more softly than usual), its effect, if any, would be distributed over children of all ages.

A more serious difficulty was to prevent the children from being influenced by what they heard other children saying. In view of this they were separated as far as possible, and were instructed to whisper and to say 'I like it' or 'I don't like it' instead of 'yes' or 'no,' for 'yes' was likely to be heard by others near, even when whispered. Generally this rule was obeyed well, but very occasionally a faint 'yes' was audible. Here again however we know something of the likelihood of the effects of suggestion, if such there were. Seeing that the youngest children are more subject to suggestion than the older ones, they would be more likely to adopt the answers of their seniors than *vice versa*, and thus would tend to raise the apparent degree of development of the juniors. As will be seen, this possibility only makes some of the later observations more significant. The question as to whether the youngest children completely understood what they had to do will be discussed later. I may say here that though one or two of the

youngest children did not give any answer with respect to some of the intervals, none of the answers given were foolish in the sense of being inapplicable, *i.e.* they were always judgments as to whether they liked the notes or not. In very few cases was the answer 'I don't know' given—indeed it might have been more encouraging if this answer had been given more often.

VI. *Results of the experiments with the Elementary School children.*

An interval scored +1 when the judgment 'I like it' was given, -1 for the judgment 'I don't like it.'

In Table V are given the votes of the children of various ages for the different intervals. As the numbers of children of the various ages were unequal, the votes are adjusted to make them represent proportionately the judgments of thirty children of each age, for the sake of easy comparison. The intervals are arranged in their order of preference, as determined by adults. For the sake of comparison a column is added showing the votes given to the intervals by the 146 adults, reduced to represent the votes of thirty individuals.

TABLE V. *Showing average votes for thirty individuals of all ages.*

As each child judged each interval twice, the highest possible score for any interval is +60 and the lowest possible -60. But these figures do not apply to the adults owing to greater variations of judgments which were permitted to them [*e.g.* very pleasing +2, very displeasing -2, slightly pleasing + $\frac{1}{2}$, and so on].

Age in years	6	7	8	9	10	11	12	13	Adults
Actual no. of children ...	15	24	25	27	27	24	22	31	
Major third	36	37	44	36	33	41	35	37	36
Minor third	38	35	35	42	38	36	20	25	29
Octave.....	26	29	37	42	31	42	22	31	27
Major sixth	20	22	35	42	31	40	28	32	27
Minor sixth	32	36	36	29	33	25	28	37	24
Fourth	34	35	26	36	36	37	25	-2	17
Tritone	28	40	28	33	23	17	5	12	17
Fifth	22	41	35	22	29	35	0	11	15
Major second.....	32	42	20	16	6	9	-11	-5	-11
Minor seventh	24	30	40	28	18	0	3	2	-18
Major seventh	28	35	26	21	9	-20	-20	-25	-35
Minor second.....	16	30	20	4	4	-5	-35	-32	-41
Av. score of 8 concords...	27.0	33.1	34.5	35.0	31.7	33.5	20.4	22.9	24.0
„ „ 4 discords ...	25.0	35.6	26.5	17.0	9.2	-4.0	-15.8	-15.0	-26.2

An inspection of Table V shows that there is no consistent preferences for consonances over dissonances at the ages of six and seven. With the six-year-olds two of the discords are liked more than the octave, and three of them are liked more than the fifth or the major sixth. At seven years all the discords are liked better than the octave or minor sixth.

Of course one cannot infer that there is no capacity at this age to discriminate between a discord and a concord. There may be a difference of sense experiences in the case of these children which corresponds to the different experience we have in the consonances and dissonances. It is more than likely that the well-known love of children for noise of any kind keeps in the background any tendency to dislike a discord as such¹. Indeed, their love of sound for its own sake may act in favour of discords. For as Stumpf has shown, dissonant intervals often appear to children as containing three, four or even five notes, apparently giving a greater body of sound, and even to adults they are generally more stimulating, in a sensational way, than are consonant intervals.

We can, however, at least infer that at this age these children had not such unpleasant sensations produced by discords as to diminish appreciably their pleasure in the sound as sound.

There remains the difficulty as to whether the children really understood what they had to do. But surely the question 'Do you like that?' should be intelligible to the average child of six or seven *if there is any definite feeling of pleasure or displeasure produced*. Where there is precocious development of musical sensitivity, children are able at an even earlier age than this to express very definite and decided judgments upon intervals and chords, as in the case quoted by Stumpf, of a five-year-old boy who could 'sing seconds' to a melody with ease, and who always gave an immediate judgment in favour of the major trichord as compared with the minor, whichever was played first². The fact that occasionally a child would say 'I like it' (or still more rarely 'I don't like it') to almost every interval, might appear to indicate a lack of comprehension as to what was being done; and this may have been the case with a few of the least intelligent pupils. But it seems quite possible that even in these cases the uniformity of judgments signifies an inability to appreciate the contrast between

¹ Cf. Dr Myers's remarks upon the dangers of inferring that some primitive tribes have no feeling for consonance because they disregard it in their music. "The Ethnological Study of Music," in *Anthropological Essays* presented to E. B. Tylor, Oxford, 1907, 239.

² *Tonpsychologie*, II. 378.

consonances and dissonances; for series of judgments, all of them 'pleasing,' were also given by children of ten, eleven, twelve and thirteen years, and such series can hardly be ascribed to incapacity to understand at this age.

Nor do the more intelligent children show any greater antipathy to the discords than do the less intelligent¹. Unfortunately the numbers are very small when one comes to divide the children of each year into intelligent and unintelligent. But I may state that the average score for the four discords among twenty-two 'intelligent' children of six or seven years of age was 0.5, that of the concords being 0.6². For eight comparatively unintelligent children the scores were as follows: discords 0.65, concords 0.7. Among the eight-year-olds, indeed, it was the thirteen unintelligent children who showed a preference for the concords, the seven intelligent ones showing a slight preference for the discords. Thus there is no evidence that, with these children, capacity to understand the simple requests made of them was the main factor in determining the trend of the scores, or, indeed, that it had any such influence whatever³.

If we take the children individually, we find that none of the six- and seven-year-olds give even one judgment of dislike to each of the four discords without also giving such judgments freely among the concords, including the octave. At eight years, however, we find two children who only give three of eight possible votes of approval to the discords, but give respectively fourteen and thirteen (of sixteen possible) to the concords.

At nine years of age we find a great advance. At least seven children,

¹ My best thanks are due to Mr J. Williamson, Headmaster of South Tay Street School, Dundee, for the care he took in personally classifying all the children in his school who underwent these tests. In cooperation with the class teachers he divided them into three groups, 'intelligent,' 'moderate,' and 'weak.' Owing to the smallness of the last group they are reckoned with the 'moderates.' I did not obtain such a classification from the other school from which the children were taken, chiefly because of the impossibility of making sure that the same standard of intelligence was being used as a basis of division; moreover, the number from the second school was too small to make separate calculation of any value.

² The proportions of intelligent and unintelligent children were approximately equal for both these ages.

³ There is on the other hand no absolute *proof* to the contrary: it might still be the case that the task was so much above the powers even of the most intelligent eight-year-old children that even their superior intelligence would not be of any value to them or cause any contrast between their results and those of the less intelligent. But this seems highly improbable.

out of twenty-seven, showed a marked and consistent preference for the concords before the discords. Their totals are :

‘Pleasing’ judgments given upon four discords, 16
 ” ” ” ” eight concords, 93.

This great advance at the age of nine is also reflected in the totals for all the nine-year-olds, the average score of the discords being now only one-half that of the concords.

Summing up, then, we may say that, on the evidence of 936 votes, no preference for concords before discords is shown by the six- and seven-year-old children ; that a slight preference for the concords begins to appear among a few of the eight-year-old children, calculated on a basis of 600 votes ; and that at nine years old the preference for concords is decided. It is interesting to note that Mr J. A. Gilbert as a result of his experiments on “The Musical Sensitiveness of School Children¹,” concluded that in the discrimination of tones varying in pitch, the average school child improves more than twice as fast from six to nine years, as it does in the years from nine to nineteen.

At the age of ten the discords become still less pleasing, and at eleven they have a negative score for the first time, though the score of the concords is practically as high as ever. At twelve and thirteen the children become much more critical in their attitude both towards the discords and to those concords which were found to be least liked by the adults, viz. the fourth, the tritone and the fifth. In regard to intervals about which adults have been seen to differ so much in their judgments, one cannot expect a perfect regularity of change in the votes of the children with increasing age. But on the whole we may say, judging from Table V, that no marked and continued preference is shown for any of the concords, and none of them is conspicuously less pleasing than the average, until we come to the ages of ten and eleven, where the lesser consonance of the tritone seems to have its effect upon the children. At twelve and thirteen, however, we suddenly find a comparative indifference to the highly consonant fifth and fourth, similar to what was found with adults. This is shown in Table VI, where the scores of six- and seven-year-old children are added together, and also those of eight and nine and so on, and then reduced to represent the votes of thirty children. The votes of the men and women are also given, adjusted so as to make the votes given for the most pleasing interval approximately the same as the children’s vote.

¹ *Studies from the Yale Psychological Laboratory, 1892-3.*

TABLE VI. *Showing average votes for thirty individuals.*

Ages	6 and 7	8 and 9	10 and 11	12 and 13	Men	Women
Major third	36	40	37	36	35	37
Minor third	36	38	37	22	26	31
Octave	27	39	36	26	30	26
Major sixth	21	38	35	30	26	28
Minor sixth	34	32	29	32	26	22
Fourth	34	31	36	11	16	19
Tritone	34	30	20	8	17	17
Fifth	31	28	32	5	15	16
Major second	37	18	7	-8	-10	-12
Minor seventh	27	34	9	2	-17	-19
Major seventh	31	23	-5	-22	-30	-39
Minor second	23	12	0	-33	-38	-43

By the age of twelve or thirteen then, these children have reached a stage at which their preferences for the various intervals are remarkably like those of the men and women. The major and minor thirds, the octave, and the two sixths form the 'most pleasing' group, both for these children and for the adults, the major third leading in all three columns. The fourth, fifth and tritone form a group of 'less pleasing' (but still 'pleasing') intervals, while the discords all have minus scores except the minor seventh which just secures +2. It is curious that this interval is the most pleasing (or least displeasing) of the discords for children from the age at which any discrimination between concords and discords takes place, while for the adults the major second is preferred of the four discords.

VII. *Results of experiments on Preparatory School children.*

Table VII shows the results of the experiments in the girls' Preparatory School. These children wrote their own judgments on paper, with the exception of one or two of the very youngest, for each of whom one of the teachers did the writing. They were taken in four groups of about eighteen each, including children of various ages. In a few cases the children modified their judgments by 'very,' etc., saying 'very pleasing' (or 'displeasing') or 'slightly pleasing' (or 'displeasing'), for which the scores of $1\frac{1}{2}$ (or $-1\frac{1}{2}$) and $\frac{1}{2}$ (or $-\frac{1}{2}$) were awarded. These children took a very keen interest in the tests, and most of them, even the very youngest, seemed very decided in their judgments.

TABLE VII. *Preparatory School Results. Showing average votes for thirty individuals of all ages.*

Ages	6 & 7	8	9	10	11	12	13 & 14
Actual no. of children...	7	10	11	10	9	15	14
Major third	45	57	52	43½	56	56	43
Minor third	45	40	30	54	53	51	44
Octave	4	45	14	49½	41½	50	52
Major sixth	17	45	36	33	51½	40	43
Minor sixth	0	42	18	39	41½	45	25
Fourth	40	42	3	18	7½	30	26
Tritone	-13	33	17	21	24½	26	11
Fifth	10	39	1½	6	8	38	7½
Major second	-34	-18	-47	-36	-50	-10	-43
Minor seventh	6	24	-44	-36	-23	-36	-36
Major seventh	-38	-24	-58	-42	-60	-58	-53
Minor second	-21	-45	-60	-54	-61½	-60	-58

N.B. As these children had permission to say 'very pleasing' and 'very displeasing' (scoring $+1\frac{1}{2}$ and $-1\frac{1}{2}$ respectively) the maximum and minimum scores for each interval are $+90$ and -90 respectively, instead of $+60$ and -60 , as was the case with the elementary children.

The scores are raised to represent proportionate numbers for thirty children in each column for the sake of comparison between the different ages and with Table V.

It will be seen at once that we have strikingly different results from those given in Table V. At eight years of age, and even at six and seven, the discords are already discriminated and all have minus scores, with the exception again of the minor seventh, which, as with the Elementary School children, retains its power to please longer than the other discords. By the age of nine we have preferences very similar to those of the adults, and such as are not given by the Elementary School children before the ages of twelve and thirteen, viz. the major third leading, the fourth and fifth low, and all the discords with negative scores.

Of course, seeing that the intervals were only presented twice to each subject, the numbers of children are very small on which to base any conclusion, and some variations occur which we might expect under the circumstances. From the first, however, a marked antipathy to the discords is shown. Possibly for a thoroughly reliable average we require a number given by the addition of not less than three of the columns of

Table VII. But an order of preference remarkably like that of the adults is given either by adding the eight- and nine-year columns or those of the nine- and ten-year-olds, as is shown in Table VIII.

TABLE VIII.

Children of 8 or 9 years.	Adults.	Children of 9 or 10 years.
1. Major third.	1. Major third.	1. Major third.
2. Major sixth.	2. Minor third.	2. Minor third.
3. Minor third.	3. Octave.	3. Octave.
4. { Minor sixth.	4. Major sixth.	4. Minor sixth.
{ Octave.	5. Minor sixth.	5. Major sixth.
6. Tritone.	6. Fourth.	6. Tritone.
7. Fourth.	7. Tritone.	7. Fourth.
8. Fifth.	8. Fifth.	8. Fifth.
9. Minor seventh.	9. Major second.	9. Minor seventh.
10. Major second.	10. Minor seventh.	10. Major second.
11. Major seventh.	11. Major seventh.	11. Major seventh.
12. Minor second.	12. Minor second.	12. Minor second.

VIII. *Comparison of the results of the experiments in the Elementary and Preparatory Schools.*

Table VIII shows that the Preparatory School children at about the age of nine show a resemblance to the adults in their liking for the different intervals, which is only shown at the age of thirteen by the Elementary School children.

This great difference must be attributed presumably to one or more of the following grounds: (i) greater intelligence, (ii) greater inherited sensitivity to music, (iii) much earlier and more thorough instruction in music and closer familiarity with good music. We have good grounds for disbelieving that general intelligence has much to do with the results of these music tests. The absence of any consistent difference between the judgments of the more intelligent and the less intelligent of the six-, seven- and eight-year-old children in the Elementary School has already been mentioned (p.204). Among the older children also there is absolutely no regular tendency for the intelligent children to approximate to the adult standard any more than the unintelligent do.

Doubtless, then, one or both of the last two causes mentioned are responsible for the difference between the Elementary and Preparatory School children. Unfortunately these experiments do not afford decisive evidence as to which is the chief cause. After the age of seven practically every girl in the Preparatory School learned some musical

instrument, and they all often heard good music¹. Many of the older children had been learning music for five or six years; while among the Elementary School children only four boys and eleven girls had had any lessons on the piano, and two (girls) on the violin. Thus the difference in musical training among the two sets of children is enormous. As to how far this is accompanied by greater inherited musical capacity it is difficult to say, but it seems to me probable that this latter has comparatively little to do with the observed differences between the two types of schools. The comparative ease with which the children of the upper classes, as compared with those of the lower, take to music, even if clearly demonstrated, would not help us in deciding the relative importance of heredity and training, for the former have already been more accustomed to hear good music than the latter.

Both Prof. Spearman and Mr Burt found that children of more cultured families had greater powers of pitch discrimination than those of less cultured families, the average thresholds in two Preparatory Schools being little more than half those attained from Elementary School children, a difference which it was shown could not be attributed to practice².

We are, I believe, ignorant of the extent to which sensitivity of pitch discrimination affects the aesthetic appreciation of consonance. But we shall see later (Table IX) that those Elementary School children who did well in two of Stumpf's tests for a 'musical ear' (cf. p. 210) reached a stage of development equivalent to that of the adults at an earlier age than those who did badly in these tests. At least we can assume that a fairly keen discrimination both of pitch and of consonances from dissonances is necessary for a degree of general musical ability much above the average. Thus they would be selected together in at least one of the ways in which we may suppose the average musical

¹ Of the seven children under eight years of age, three (two of seven years of age and one of six years) had not studied music. They showed scarcely any preference for concords over discords. The four who had studied music (three of seven years and one of six years) showed much more discrimination, the scores giving an average of nearly 0.5 per vote for the concords, and -0.2 for the discords. But these numbers are of course far too small to base any inference upon. Apart from this there is doubtless a tendency for the precociously musical child to begin lessons earlier than the others. From the eight-year-old son of a University Professor, well above the average in intelligence, and who had had no instruction in music, I was able to obtain four judgments upon each of the intervals. He showed practically no preference on the whole for concords (average vote 0.6) to discords (average vote 0.5). There was however a marked preference for the major and minor sevenths (average of each +1.0, i.e. 'pleasing') to the major and minor seconds (average -0.25 and +0.25 respectively).

² See this *Journal*, III. 125.

ability of the upper-middle classes to have been increased or maintained, and that of the lower classes comparatively lessened, namely by the selection and raising to a somewhat higher social standing of persons of marked musical ability in the lower-middle or working classes. Further speculation on this point is beyond the scope of this paper. But granted some difference in the musical sensitivity of the two social groups, few will doubt that their musical training was an important factor in determining the rapid advance in the Preparatory School children in the capacity to appreciate the difference between consonances and dissonances. One possible criticism must be dealt with. It may be suggested that to the young children who had had musical instruction the discords may have sounded 'wrong' without having any of the unpleasantness naturally associated with dissonance. But that implies a capacity for remembering and identifying an interval which the music mistress had frequently corrected, a capacity which is surely more surprising and improbable in young children than the feeling of the unpleasantness of a discord. Even a musical adult in these experiments mistook a major second, which he found pleasing, for one of the thirds, and was doubtful whether a pleasing minor seventh was not an octave. Nor do the reasons given by the children give any support to this suggestion. The only one who speaks of the notes 'sounding right' is an Elementary School boy of seven years who had not had music lessons.

IX. *Tests for a 'musical ear.'*

At each sitting, after the playing of all the intervals and before they were repeated in the opposite order, other simple musical tests were given, similar to those used by Stumpf, for discriminating musical from unmusical individuals. In test A the children were asked to say whether I was playing one or two notes on the piano. Three times two notes were played (the octave, major third and tritone), three single notes being interspersed. In test B, the children had to say whether the second of two notes, played successively, was higher or lower than the first. Three times the second note was higher, and three times lower than the first. In four cases the notes differed by a semitone and in two cases by a full tone. I first explained what higher and lower meant by playing a succession of ascending notes on the piano and saying that those were 'getting higher,' and similarly with a succession of descending notes. As test A is appreciably harder than B, especially

to children unfamiliar with the piano, mistakes in A were only reckoned as half errors, those in B counting as full errors. In order to divide the children roughly into two equal groups, and to allow fully for 'slips' and misunderstandings, only those whose total errors were more than two were classed as 'unmusical.' The number of boys taking part in the experiments was 95, the girls numbering 100. Of these 37 boys and 43 girls were 'musical,' numbers that do not allow us to infer that either sex, at this age, is more 'musical,' as far as these tests can indicate.

No marked correlation was observable between success in these musical tests and general intelligence, a result which encourages the belief that the task was explained clearly enough even for the duller children to understand. The numbers were as follows:

<i>Intelligent children.</i>	<i>Unintelligent children.</i>
31 musical, 42 unmusical.	26 musical, 38 unmusical.

Table IX shows the connexion between success in these tests, and the votes for the various intervals. As there are only four musical children of seven years we will ignore that column. An examination of the others shows that among the younger children (ages eight and nine) the musical ones are more critical throughout, but not more averse, *proportionately*, to discords than are the unmusical children. At ten years the musical children are slightly less critical both towards concords and discords.

Thus up to the age of eleven or thereabouts, the greater sensitivity of ear, as shown by the musical tests, does not result in greater discrimination of concord and discord. There is, on the average, a slightly more critical attitude towards all intervals, possibly the beginnings of an attitude in which the notes are no longer pleasing merely because they are sounds, an attitude perhaps more readily adopted by the child possessed of a more sensitive hearing (in a musical sense). But after eleven years the more 'musical' children are emphatically more averse to discords than are the 'unmusical,' though at least as appreciative of concords. The total scores for the discords (for thirty children) are 15.3 for the unmusical, but - 89 for the musical children over eleven years of age. This marked correlation must mean either that a musical ear (as tried by Stumpf's tests) does eventually reveal itself also in greater sensitivity to the unpleasantness of discords, or that the two capacities, though not directly connected, are generally found together. No doubt familiarity with the piano would be one factor in determining success in Stumpf's

TABLE IX. *Votes of 'musical' and 'unmusical' children.*

Ages	13		12		11		10		9		8		7	
	Musical	Unmusical	Musical	Unmusical	Musical	Unmusical	Musical	Unmusical	Musical	Unmusical	Musical	Unmusical	Musical	Unmusical
No. of children	22	9	10	14	14	10	10	16	14	13	7	18	4	20
Major third ...	32	6	14	12	23	10	12	18	10	22	8	29	6	24
Minor third ...	22	4	10	4	16	13	8	26	16	22	7	22	4	24
Octave.....	19	13	4	12	24	10	16	16	18	20	7	24	2	21
Major sixth.....	27	7	8	12	16	16	12	16	16	22	3	26	2	16
Minor sixth.....	24	14	2	18	12	8	12	18	10	16	8	22	2	27
Fourth	-2	0	6	12	16	14	14	18	10	22	8	14	6	22
Tritone	11	1	0	4	10	4	11	10	16	14	5	18	6	26
Fifth	4	7	2	-2	20	8	8	18	10	10	8	21	6	27
Major second...	-10	5	-6	-2	-9	16	2	2	8	6	-1	18	8	26
Minor seventh..	0	2	-8	10	-6	6	8	8	6	19	7	26	6	18
Major seventh..	-22	-4	-10	-4	-18	2	4	4	12	7	8	14	2	22
Minor second...	-28	-5	-10	-16	-6	2	2	2	0	4	5	12	4	20

Total votes, adjusted to represent votes of 10 children—

8 concords	61·8	57·7	46	51·3	97·8	83	93	87·5	75·7	113·8	77·1	103·3
4 discords	-27·2	-2·2	-34	-8·5	-27·8	26	16	10	18·5	27·6	27·1	38·8
	The musical children are much more averse to discords		The musical children are much more averse to discords		The musical children are much more averse to discords		No very decided difference		The musical children are more critical towards <i>all</i> intervals		The musical children are more critical towards <i>all</i> intervals	

tests¹. If so, we should expect to find this correlation, on the assumption which we have already made, that the aesthetic appreciation of consonance is also developed by familiarity. But it is difficult to explain the marked correlation among the older children by this familiarity with the instrument alone, for nearly all these Elementary School children gain their knowledge of pianoforte music largely from the school, where they have equal opportunities. Very few would have a piano in the home, and as we saw only 17 out of 195 were having music lessons.

It seems highly probable, then, that the musical sensitivity, as tried by Stumpf's tests, is closely connected with the aesthetic discrimination between consonance and dissonance, both doubtless being cultivated by

¹ The same tests were given to the Preparatory School girls but only 7 of the 76 made a total of more than two errors, and 3 of these were only 7 years old. All of them, except one seven-year-old, were as averse to the discords as were the 'musical' children.

familiarity with music. The average improvement in Stumpf's tests is especially rapid up to, and including, the age of nine, as Gilbert found was the case with tone discrimination. That it is not accompanied by greater discrimination between concords and discords until a year or so later may signify that further musical experience is necessary before the improved capacity for discrimination (such as is involved in Stumpf's tests) can be brought to bear on the higher aesthetic appreciation of consonance or dissonance¹.

X. *Introspection of school children.*

(a) *Elementary School children.*

Naturally the records were not rich in introspective remarks, although the children were asked to give their reasons for liking or disliking the intervals. Such terms as 'sweet,' 'good,' 'nice' were common with the youngest children. The first indications of the hearer's attention being attracted to the effect of the notes upon *himself* occur at the age of nine and are then concerned wholly with unpleasant effects, e.g. 'makes my head ache,' 'makes an awful sound in my ears,' 'makes my ears ring.' At the ages of eleven, twelve and thirteen we find more frequent judgments of this kind, and no longer confined to unpleasant effects, thus: 'pleasant to the ear,' 'soothing,' 'grating,' 'nice for singing to.'

Children over the age of eleven also make the comments 'bold,' 'cheery,' 'strong,' 'awful sad' and 'uncanny.' Frequent references are made to the pitch of the notes as the following list shows.

Intervals said to be

A	B	C
Pleasing because high, by 15 boys and 15 girls	Pleasing because low, by 4 boys and 4 girls	Displeasing because too low, by 20 boys and 24 girls

Six of the eight children who find intervals pleasing because 'low' are over twelve years of age. In column A, twenty-three of the thirty children were over ten years of age. In column C, thirty-nine of the forty-four children were over ten, the numbers being fairly equally distributed (in the case of both columns A and C) among the various ages from ten to thirteen. Thus the height of the pitch does not appear to effect the younger children any more than the older ones.

¹ I do not think that anything said here is inconsistent with Mr T. H. Pear's criticism of these tests as adequate tests of general musical capacity (this *Journal*, iv. 89), with which I fully agree. Even the addition of a test for the appreciation of consonance and dissonance would not make a satisfactory test of the capacity to *enjoy* music.

Some increase after the age of nine in the number of children who refer to height or lowness of pitch is only to be expected, as the children become more able, with increasing age, to give reasons for their likes and dislikes. The answers as a whole indicate that somewhat higher intervals would have been more pleasing to the children. The pitch used would be low for the children to sing, and some introspective remarks suggest that this is of considerable weight in determining the pleasingness of a note to a child.

The occurrence of associations is peculiar. They occur far more frequently about the ages of ten and eleven than either before or after those ages. Thus 106 associations out of 165 are given by children of ten or eleven years of age, *i.e.* nearly two-thirds of the associations are given by about one quarter of the children. Twenty-three boys give associations, but only thirteen girls, though the average number of associations given by these thirteen is higher than that of the twenty-three boys¹. Only two girls but ten boys under the age of ten give associations. These facts may indicate a greater interest on the part of young boys (compared with girls) in the sounds of objects suggested by the intervals, *e.g.* bells, clocks, motor horns, and one delightful expression of discord on the part of a small boy, "like a man smashing a tin can and he can't smash it any more."

(b) *Preparatory School children.*

Here the most striking fact is the absence of associations. In the 1824 judgments given by these children only one association occurred, 'like a bugle.' The principal cause undoubtedly lies in the high degree of musical training of these children. They never think of associating such sounds with anything *but* a piano, an instrument they know so well.

XI. *Sex differences in the Elementary School experiments.*

We have already noted the more frequent occurrence of associations among the boys. Another marked difference is revealed by the total votes for and against the intervals. The boys are much more critical, the girls much more disposed to say 'I like it.' Thus the eight concords receive a balance of positive votes to the number of 879 from the 100 girls, but only 688 from the 95 boys. This characteristic difference is shown through all the ages. Table X shows the actual number of votes for the various ages.

¹ Chiefly owing to the fact that one girl gave fourteen associations.

TABLE X.

	Ages 6 & 7		Ages 8 & 9		Ages 10 & 11		Ages 12 & 13		All ages	
	17 boys	22 girls	28 boys	24 girls	21 boys	26 girls	26 boys	28 girls	95 boys	100 girls
Balance of votes for 8 concords	138	202	224	260	197	252	129	165	688	879
Balance of votes for 4 discords	66	94	51	100	27	- 8	- 68	- 50	76	136

In each column the boys and girls are distributed between the two ages (given at the head of the column) in similar proportions, except in the first column where there are 14 girls of seven years to 8 of six years, and only 10 boys of seven years to 7 of six years.

The totals for all ages suggest that the boys as a whole discriminate or at least dislike the discords more than do the girls. But the difference is almost entirely traceable to the ages of eight and nine where it is very marked.

XII. *Summary of results and conclusions.*

I. The apparent pitch of an interval is for most people determined approximately by the pitch of its higher note, and not of its lower note as has been previously asserted.

II. Of all the intervals used the major third was by far the most liked by adults. Then comes a group of four, viz. minor third, octave, major and minor sixth, which on the average are found 'pleasing.' Then a third group, the fourth, tritone and fifth, each on the average 'slightly pleasing.' This order, it will be observed, is by no means coincident with the order of degree of consonance.

III. The major third and major sixth are described as sad by adults twice as often as the minor third and minor sixth. This supports the view that the usual effects of the minor key for modern European ears are not due to any 'natural' effect of the minor intervals, but are determined by association.

IV. Even the highly consonant intervals of the fifth and fourth are sometimes described (by adults) as discordant.

V. Among the children in the Elementary Schools tested no appreciable preference for concords before discords is discernible before the average age of nine, at which age a considerable advance takes place.

VI. A group of children of the age of twelve or thirteen gives an order of preference for the twelve intervals within the octave, which is remarkably like that given by adults.

VII. No appreciable difference is discernible between the preferences of the more intelligent and those of the less intelligent children.

VIII. No correlation appears to exist between general intelligence and the capacity for such simple tests as comparing the pitch of two notes, or detecting whether one or two notes are being played on the piano at a time.

IX. Greater musical capacity, as measured by such tests, is correlated (but only after the age of eleven) with much greater aversion to discords.

X. Associations occur with musical intervals most frequently by far at the ages of ten and eleven.

XI. More boys than girls have such associations. On the average boys are much more critical than girls towards musical intervals.

XII. The Preparatory School girls show an aversion to discords (except to the minor seventh) even at the ages of seven and eight, and about the age of nine they give, on the average, an order of preferences for the twelve intervals very similar to that given by adults. Thus, by the age of nine, they reach a stage of development only attained by the Elementary School children by the age of twelve or thirteen¹.

¹ My thanks are due to Dr C. S. Myers for a careful reading of this paper and for the suggestion of several emendations; also to Miss Preston, Headmistress of St Catherine's School, St Andrews; Mr J. Williamson, Headmaster of South Tay Street School, Dundee, and Mr A. Swinton, Headmaster of Balfour Street School, Dundee, for the facilities they kindly afforded me for testing their pupils in these experiments.

(Manuscript received 20 September, 1912.)

NOTE ON THE PROBABLE ERROR OF URBAN'S FORMULA FOR THE METHOD OF JUST PERCEPTIBLE DIFFERENCES.

By GODFREY H. THOMSON,

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THE object of this note is to correct an error in Urban's application of Bernoulli's Theorem for the calculation of the probable error of the Method of Just Perceptible Differences¹, which was followed by the writer in an article on "The Best Form of the Method of Serial Groups²." Fortunately the conclusions drawn in both articles are not seriously affected by the correction.

By the Method of Just Perceptible Differences Urban really means a process of calculation (the Limiting Process³), which can be applied to data collected by several psychologically different methods. The full calculations as carried out by Urban can, however, only be performed if a large number of experiments have been made with each stimulus value: he himself in the article quoted applied the Limiting Process to data collected by the Method of Right and Wrong Cases⁴ with lifted weights. The standard weight was 100 grams, and was lifted before each of the seven comparison weights, which were so chosen that the subject nearly always recognised the extreme weights as lighter or heavier respectively than the standard. The experimenter presented the comparison weights to the subject in an irregular sequence, and the whole series was repeated 450 times. The judgments given were *lighter*, *equal*, or *heavier* than the standard. The answers were entered in a

¹ F. M. Urban, "Die Psychophysischen Massmethoden als Grundlagen empirischer Messungen," *Arch. f. d. ges. Psychol.* 1903, xv. 261-415.

² This *Journal*, 1913, v. 398-416. The writer will be obliged if readers, in referring to this article, will note this correction.

³ G. H. Thomson, "Comparison of Psychophysical Methods," this *Journal*, 1912, v. 210.

⁴ See Thomson, *op. cit.* 204.

table containing 450 horizontal rows and seven vertical columns corresponding to the seven comparison weights.

Provided with this table, the calculator applies the Limiting Process as follows. Beginning at the left-hand end of each row with the weight 84 grams, he passes along until a judgment *heavier* is met. The weight at which this occurs is noted as one reading of the just perceptible positive difference. For some reason which I cannot discover, Urban only uses 400 rows of the table obtaining that number of readings which in the case of his Subject I. were distributed as follows¹:

TABLE I.

Comparison weights r	84	88	92	96	100	104	108
Frequencies N	0	7	30	76	106	169	12

The mean of all these readings is 100.36 grams, which is taken as the mean just perceptible positive difference, and it is the probable error of this number which is required. Before examining Urban's processes for finding this, we may see approximately what it must be. The "quartiles" of the above distribution occur at 96 grams and 104 grams, the semi-interquartile range is 4 grams and the probable error of the mean is therefore about

$$\frac{4}{\sqrt{400}} = 0.2 \text{ gram.}$$

Urban² indicates three processes for finding this quantity more accurately. One of these he does not recommend or use. It is the ordinary algorithm of Least Squares, and gives about 0.16 gram. The two formulae which Urban does use are based on an inverse use of Bernoulli's Theorem, and are both incorrect³. They give values 2.832 grams and 2.373 grams respectively, more than ten times too large.

In his first formula Urban assumes that with each of the comparison weights r_k there is associated a probability P_k that it will be recorded as a reading of the just perceptible positive difference. The frequencies N in Table I, when divided by 400, are experimental determinations of

¹ Urban, *op. cit.* 322, Table 18.

² Urban, *op. cit.* 313-317.

³ I am indebted to Professor Karl Pearson for confirmation of this statement.

these probabilities. The inverse use of Bernoulli's Theorem gives as the probable error of P_κ

$$\omega_\kappa = \cdot 6745 \sqrt{\frac{P_\kappa(1-P_\kappa)}{s}} \dots\dots\dots(1),$$

where s is the total number of experiments (400). The mean just perceptible difference T is

$$T = \frac{\sum_1^n N_\kappa r_\kappa}{s} = \sum_1^n P_\kappa r_\kappa \dots\dots\dots(2).$$

Therefore (and this is where the mistake occurs) the probable error F of T will be given by

$$F^2 = \sum_1^n \omega_\kappa^2 r_\kappa^2 \dots\dots\dots(3).$$

But this last step would only be correct if the P 's were independent of one another. By the nature of their formation, however, their sum is necessarily unity. The largest possible value for ΣPr is therefore 108 grams and the smallest possible value 84 grams, a range of 24 grams. Were the P 's independently measured, their sum would not necessarily be unity. Independent measurement would mean that in one set of experiments, using all the weights, we would ascertain how often the weight 84 grams was noted as a just perceptible positive difference and how often it was not so noted, and nothing else. Then in *another* set of experiments we would do the same for 88 grams, and so on for each weight. Now the chances might possibly be against each weight in turn just as we were doing the set of experiments which were focussed upon it; or on the other hand the chances might be in favour of each weight in turn just at the right time. In the first case each P might even be zero, which would give a value zero for the threshold ΣPr . In the second case each P might even be unity (except the P at 84 grams, which must in any case be nearly zero, for otherwise we would simply take a still lower weight as the beginning of our set of comparison weights). This would then give a value of 588 grams for the threshold, namely

$$\Sigma Pr = 88 + 92 + 96 + 100 + 104 + 108.$$

The possible range assumed by equation (3) for the threshold is therefore from zero to 588 grams. Of course no experimenter would accept such results, but Urban's formula assumes their possibility. Were experiments really made independently, they would be continued until ΣP approximated to unity and then the values would be adjusted

as are the angles of a closed polygon in a survey. We may expect therefore that Urban's probable error 2.832 will be too large in something like the proportion of these two ranges and that the correct value will be approximately of the order

$$\frac{108 - 84}{588 - 0} \times 2.832 = 0.12 \text{ gram,}$$

a value much more in accordance with what we found by Least Squares¹.

Urban's other process is based upon the actual number of answers *heavier* recorded for each comparison weight. These were as follows in 450 trials²:

TABLE II.

Comparison weights r	84	88	92	96	100	104	108
Answers <i>heavier</i>	1	9	40	100	186	403	423

Urban now assumes that associated with each comparison weight r_κ there is a probability p_κ that the subject will answer *heavier*. The numbers in Table 2, when divided by 450, are experimental determinations of these probabilities. Each is therefore subject to a probable error

$$\omega_\kappa = .6745 \sqrt{\frac{p_\kappa q_\kappa}{s}} \dots\dots\dots(4),$$

where

$$s = 450 \text{ and } q = 1 - p.$$

The former probabilities P can be calculated from the p 's and then the threshold T can be found from them. Since therefore T is ultimately compounded of the values p , it ought to be possible to calculate its probable error from the probable errors of the p 's; and since these latter probabilities are quite independently measured, the objection previously raised does not hold here. Unfortunately Urban performs the algebra in two steps, calculating first the probable errors of the P 's

¹ This is not suggested as an exact or practical way of finding the probable error. The alternative to Least Squares, if only Table I is known, is indicated towards the end of this Note, after Urban's second formula has been discussed.

² Multiply the numbers in Urban, *op. cit.* 287, Table 11, column *Vp. I grösser*, by 450; or read direct from Urban, *Application of Statistical Methods to the Problems of Psychophysics*, Philadelphia, 1908, 174, Table 3.

and from these that of T , thus reintroducing the same mistake. His second equation on page 316 *op. cit.* is incorrect. It assumes that

$$\frac{dT}{dP_\kappa} = r_\kappa \dots\dots\dots(5),$$

which is not true. The correct formula is

$$F^2 = 2\rho^2 \sum_1^n \left(\frac{dT}{dp_\kappa} \right)^2 \frac{p_\kappa q_\kappa}{s_\kappa} \dots\dots\dots(6),$$

where $\sqrt{2\rho^2} = .6745$.

After performing the differentiation, and remembering that q_n and p_1 must nearly equal zero, we get

$$F = .6745 \sqrt{\sum_{\kappa=1}^{\kappa=n} \frac{p_\kappa}{s_\kappa q_\kappa} \left(\sum_{\lambda=\kappa}^{\lambda=n} P_\lambda r_\lambda - r_\kappa \sum_{\lambda=\kappa}^{\lambda=n} P_\lambda \right)^2} \dots\dots\dots(7),$$

a much simpler formula to calculate than Urban's. It gives in the present case the value 0.133 gram approximately. This is the formula for just perceptible positive differences, that is for ascents. For descents interchange p and q , and the suffixes 1 and n . Similar formulae hold for the negative differences.

Urban's conclusion that the Limiting Process of calculation is, for what it attempts, more accurate than calculation by the $\Phi(\gamma)$ hypothesis is of course not altered by this correction: the accuracy is even greater than he supposed, and is hardly distinguishable from that of the Lagrange interpolation formula,—as might be expected, since both processes are alike in accepting the data as given and in finding the fifty per cent. point without any attempt at smoothing the curve.

The writer, in the article cited above on the Method of Serial Groups, fell into the same error. The Limiting Process is an extreme form of the Group Process, and the writer checked his equation IV¹ by seeing that for certain values it reduced to Urban's equation. The correct form of equation IV is

$$F = .6745 C \sqrt{\sum_{\kappa=1}^{\kappa=n} \frac{p_\kappa^{2t+1} q_\kappa^{2g-2t-1}}{s_\kappa l_\kappa^2} \left(\sum_{\lambda=1}^{\lambda=\kappa} \mathfrak{P}_\lambda' r_\lambda - r_\kappa \sum_{\lambda=1}^{\lambda=\kappa} \mathfrak{P}_\lambda' \right)^2} \dots(8),$$

where the letters have the same meaning as in the article quoted. For the Limiting Process, $t = 0$, $g = 1$, $l = p$ and $C = 1$; and the equation reduces to equation (7) for descents.

The equation on page 409 of the same article is also incorrect. This is Urban's case (1a) where the P 's but not the p 's are known², to which

¹ This *Journal*, 1913, v. 415, Appendix II.

² Urban, *op. cit.* 313.

he applies his first formula discussed above. If it is desired to avoid making the assumption of a certain distribution which underlies the Least Squares process, then the only way of handling this case would be to calculate the p 's from the P 's and use the observed P 's and calculated p 's in equation (7) or (8) as the case might be.

The incorrect probable errors in the writer's article are, like Urban's, too large; but the difference is not so great, because an absolute not a difference threshold is being calculated, so that the values of r are small. The correct probable errors are from one-third to a quarter of those given; and fortunately this proportion is sufficiently constant to keep the various forms of the process in the same order of merit. The two general conclusions on page 412 are therefore still correct although the advantage of small groups is weakened.

(Manuscript received 30 August, 1913.)

THE EFFECTS OF 'OBSERVATIONAL ERRORS' AND
OTHER FACTORS UPON CORRELATION COEFFICIENTS
IN PSYCHOLOGY.

BY WILLIAM BROWN.

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University of London.)

- § 1. *The need of more careful determinations of individual correlation coefficients.*
- § 2. *A means of testing empirically the validity of Spearman's modified 'correction formula.'*
- § 3. *Preliminary results of an experimental research into the correlation of errors of measurement.*
- [§ 4. *Description of the application of two tests to a group of school-boys. By Mr W. H. WINCH.*]
- § 5. *Detailed correlation, and other, results of these tests.*
- § 6. *The inapplicability of Spearman's formula in the case of these tests. Suggestion of the best method of obtaining a reliable measure of correlation.*
- § 7. *The causes of correlation between mental abilities.*

§ 1. DESPITE the very considerable amount of careful work that has been done during the last two or three years, both in England and America, upon the correlation of mental abilities, and a greatly improved insight into the significance and requirements of the mathematical *technique* devised by Professor Karl Pearson and his school for the manipulation of statistical material, much divergence still exists in the amount of correlation found between identical abilities by different investigators and even by the same investigator at different times and with different groups of subjects. The cause of this divergence is to be found in the great complexity of factors involved in the correlation of any two mental abilities, and until these have been adequately

investigated and allowed for, speculations as to the general cause or causes of psychical correlation must be little more than futile. Instead of attempting to support or disprove anticipatory hypotheses on the basis of palpably inadequate material, investigators will now, it would seem, be better advised to adopt a more 'intensive' method of work and obtain the fullest possible insight into the conditions of application of the various mental tests and the factors influencing their performance. The time for general surveys of the entire field, with the valuable training in method which they have brought, is now almost over, and a change of tactics is needed if the present deadlock of assertion and counter-assertion is to be surmounted.

§ 2. The object of the present short paper is to deal, somewhat inadequately it is true, with one or two of the more salient difficulties of method, and so to clear the ground for the new line of investigation into mental variation which has already been commenced at King's College with the aid of a grant from the Royal Society. The main difficulty is that of the *assumptions* involved in the use of mathematical formulae. It might have been expected that such assumptions would, wherever possible, have been tested on actual psychological data. Yet those who suggest these formulae in psychology seem singularly oblivious of the necessity of this precaution. Instead of facts we are given theoretical discussion. It was, for example, the merit of Prof. C. Spearman to suggest a mathematical method of eliminating 'observational errors' in the correlation of series of mental measurements, but I was able to show¹, by empirical tests, that the assumptions upon which his formula was based were incorrect. In response to this criticism he has modified his formula², but unfortunately the new form still involves assumptions that are not empirically justified. The essence of the new method is to separate (in thought) the 'regular' deviations, which are due to such factors as practice, fatigue, habituation, etc., from the so-called 'accidental' deviations. The former, which may have correlations of their own with one another and with the two abilities whose correlation is sought, are to be eliminated by an appropriate grouping of the series of measures of the two abilities. Thus Spearman recommends that if three measures of each ability be made, the first and third may be taken together as one group, and the second as the other; if four, the first and fourth may form one group, and the second and third the other. If a larger number of measurements be

¹ *Biometrika*, April, 1910, vii. This *Journal*, Dec. 1910, iii. 320.

² This *Journal*, Dec. 1910, iii. 275.

made, the odd members of the series should form one group and the even members the other. In this way the effect of the 'regular' change of observed ability from measure to measure may be neutralised. The remaining deviations are called by him 'accidental' deviations, and on the assumption that they will be uncorrelated with one another or with the abilities to be measured, he devises a new formula for their elimination. His proof¹ of this formula may be summarised and simplified as follows:

Let x_a, y_a = average measures of one particular individual in group a for performances x and y respectively.

„ x_b, y_b = average measures of same particular individual in group b for performances x and y respectively.

„ x_{ab}, y_{ab} = average measures of same particular individual in the combined groups a and b .

Let d, e represent the corresponding 'accidental' as distinct from the 'regular' deviations from these averages.

Then, owing to the way in which the groups are chosen, we have

$$\begin{aligned}x_a &= x + d_a, & x_b &= x + d_b, & x_{ab} &= x + d_{ab}, \\y_a &= y + e_a, & y_b &= y + e_b, & y_{ab} &= y + e_{ab},\end{aligned}$$

all the measurements being deviations from their mean values.

Assume d and e uncorrelated with each other or with x or y .

Then $S(x_a x_b) = S(x^2)$, $S(y_a y_b) = S(y^2)$, and $S(x_{ab} y_{ab}) = S(xy)$.

Assume, too, that $\sigma_{x_a} = \sigma_{x_b}$ and $\sigma_{y_a} = \sigma_{y_b}$.

$$\begin{aligned}\text{Then } r_{xy} &= \frac{S(xy)}{\sqrt{S(x^2) S(y^2)}} = \frac{S(x_{ab} y_{ab})}{\sqrt{S(x_a x_b) S(y_a y_b)}} \\&= \frac{S(x_{ab} y_{ab})}{\sqrt{S(x_{ab}^2) S(y_{ab}^2)}} \sqrt{\frac{S(x_{ab}^2) S(y_{ab}^2)}{S(x_a x_b) S(y_a y_b)}}, \\&\text{which, on reduction,} \quad = r_{x_{ab} y_{ab}} \cdot \frac{1}{2} \sqrt{\left(1 + \frac{1}{r_{x_a x_b}}\right) \left(1 + \frac{1}{r_{y_a y_b}}\right)}.\end{aligned}$$

The validity of these assumptions may be tested by the same criteria which I employed in my examination of the earlier formula, viz.

- (1) $S(x_a y_a) = S(xy) = S(x_b y_b)$ within the limits of their P.E.'s,
- (2) $r_{\frac{x_a - x_b}{y_a - y_b}} = 0$ (a merely negative criterion, since it can also hold when the 'accidental' deviations are correlated).

§ 3. But before applying these criteria to some actual observations, I should like to quote here a few preliminary results of a detailed research into the correlation of errors of measurement which I carried

¹ *Op. cit.* 288, 289.

out three years ago at the suggestion of Professor Karl Pearson. Owing to the very large number of observations made, the final results are not even yet completely ready for publication. Stated very briefly, the research consisted of the estimation, by two independent observers, of the lengths of two series, X and Y , of lines ranging from 30 mm. to 170 mm. There were 50 lines in each series, and the length of every line in series Y was five-thirds of the length of the corresponding line in series X . The lines were observed from a fixed convenient distance, and the estimated lengths marked off on ruled paper and then measured. That is, the method of production was used. The 100 lines were presented in irregular order and worked through five times by each subject.

Clearly the correlation between the actual series X and the actual series Y , when the measures in each are arranged in ascending order of magnitude, is 1, and it is interesting to note how closely the observed values are correlated with one another. Taking one observer only, we have five separate estimations of series X and five of Y . We may test Spearman's formula by grouping the second and fifth together as group a , and the third and fourth as group b . This gives

$$\begin{aligned} r_{x_{ab}y_{ab}} &= \cdot99, & r_{x_ax_b} &= \cdot98, & r_{y_ay_b} &= \cdot98, \\ \therefore r_{xy} &= \cdot99 \cdot \frac{1}{2} \sqrt{\left(1 + \frac{1}{\cdot98}\right) \left(1 + \frac{1}{\cdot98}\right)} \\ &= 1\cdot0001. \end{aligned}$$

Although this result is slightly above 1, and therefore apparently impossible, it is so close to the true value that, allowing for the probable errors of the observed coefficients, we might regard it as evidence in support of the applicability of the formula. But the following product-moments refute this view, and show that the presuppositions of the formula are not obeyed:

$$\begin{aligned} S(x^2) &= 19129, & \text{but } S(x_ax_b) &= 18689, \\ S(y^2) &= 53156, & \text{,, } S(y_ay_b) &= 40052, \\ S(xy) &= 31884, & \text{,, } S(x_{ab}y_{ab}) &= 28033. \end{aligned}$$

These divergences might be partly explained as due to a tendency to underestimate the lines, but are far from being entirely explained thus, since

$$S(x_ay_a) = 24206 \quad \text{and} \quad S(x_by_b) = 30872,$$

whereas according to theory they should be approximately equal to one another.

Taking series 1 + 3 as group *a*, and 2 as group *b*, the divergences are greater:

$$S(x^2) = 19129, \text{ but } S(x_a x_b) = 15538,$$

$$S(y^2) = 53156, \quad ,, \quad S(y_a y_b) = 32770,$$

$$S(xy) = 31884, \quad ,, \quad S(x_{ab} y_{ab}) = 22210.$$

This case is of course not identical with that of measuring abilities and their correlations, but is sufficiently analogous to throw some light on the problem of 'observational errors' in correlation. Here errors of observation, which might be loosely called 'accidental' errors, are correlated with one another and with the objects measured.

I have worked out the correlation between the 500 errors of observation and the corresponding true lengths of lines for the same observer, and obtain the following results:

$$r = .033 \pm .030,$$

$$\eta_y = \sqrt{\frac{S\{n_x(\bar{y}_x - \bar{y})^2\}}{N\sigma_y^2}} = .323 \pm .027,$$

$$\eta_x = \frac{S\{n_y(\bar{x}_y - \bar{x})^2\}}{N\sigma_x^2} = .182 \pm .029.$$

Here it will be observed that whereas *r* is negligible, η_x is appreciable and η_y is very considerable. A glance at the correlation table and regression curves (see Appendix) shows the reason of this. The correlation is 'skew' and 'hetero-skedastic', and whereas the η_x curve is almost vertical and shows very little correlation, the η_y curve approximates to a Gauss curve. It is probable that much of the correlation between mental ability and errors of observation of that ability is skew or non-linear. Where such is the case, it would be very difficult to deal with, since the method of elimination by partial correlation assumes linear regression. The error-correlation in the present instance is of course at least partly due to the working of Weber's Law and could be reduced by estimating the errors as fractions of the actual lengths, but Weber's Law, or one equally non-linear, might well hold for the estimation of different forms of mental ability, and so produce equally disturbing results.

§ 4. With a view to obtaining more direct evidence upon the question of Spearman's formula, I chose two tests which I had found

¹ The scatter-diagram of a correlation table is said to be *heteroskedastic* when the standard deviations of successive rows or columns ('arrays') increase or decrease instead of remaining constant. A normal correlation table is *homoskedastic*, since here the S. D.'s of the arrays are all equal to one another.

very reliable in previous correlation research, viz. (1) marking through every letter in a page of print (Simple Motor), and (2) marking through the *a*'s, *n*'s, *o*'s and *s*'s in a similar page¹ (Complex Motor), and consulted with Mr W. H. Winch, who very kindly agreed to get these tests, together with others of his own devising, applied six times each to a group of 40 school-boys of the same age and school. The following is a detailed description of the mode of application of the tests, kindly contributed by Mr Winch, under whose direction the tests were done.

[*Description of Simple Motor and Complex Motor Tests.* By Mr W. H. WINCH.]

"The Tests were done by *all* the eleven-year-old children in a municipal elementary school for boys, situated in a rather poor district in the south-east of London. The school was a strongly-disciplined and hard-working one, and the boys might, on the whole, be relied on to give their full attention to the work, even after the influence of novelty had passed away. The Motor Tests formed part of a series consisting also of Tests in Rote and Substance Memory, in Productive Imagination and in Reasoning. The results of the whole research are not yet ready for publication, but those of the Motor Tests are now partially presented. The exercises were worked according to a definite timetable commencing on Wednesday, June 12th, 1912, at 9.45 in the morning, and on the same day at 2.15 in the afternoon, six tests being done on each day. Two days later the second series of tests was worked, then on the next Wednesday a further series, in the mornings and afternoons as before. The work continued thus on Wednesdays and Fridays until each boy had worked six tests of each kind, the concluding exercises occurring on Friday, June 28th.

Each morning's work began with a Simple Motor Test and each afternoon's work with a Complex Motor Test, each Motor Test occupying exactly five minutes. The Simple Motor Test consisted in marking out every letter from a page of print consisting of casually arranged French words; the Complex Motor Test consisted in marking out the *a*'s, the *n*'s the *o*'s and the *s*'s from similar pages. In the simple exercises one mark was given for every letter clearly marked out with the deduction of a mark for every letter omitted or doubtfully marked. In the complex exercises one mark was given for every specified letter correctly marked out, with the deduction of a mark for every omission and for every wrong letter marked. Preliminary exercises were given

¹ See *Mental Measurement*, 102; also *This Journal*, Dec. 1910, III. 300.

before the Test Series commenced, so that every boy might start with a full understanding of what he had to do. The printed papers were supplied from King's College by Dr W. Brown, and the exercises were administered by three men on the staff of the school, all of whom had had much experience of experimental work with boys.

The following is a brief summary of the results of the six Simple Motor Tests:

Simple Motor Tests. Average Marks per Boy per Test.

School Standard	No. of Boys	Wed. a.m.	Friday a.m.	Wed. a.m.	Friday a.m.	Wed. a.m.	Friday a.m.	Average of Six Tests
III	1	427.0	407.0	405.0	415.0	374.0	372.0	400.0
IV	3	444.0	456.0	519.0	556.0	573.0	573.3	520.2
V	11	470.0	506.6	561.8	620.8	626.0	651.8	572.8
VI	19	482.8	517.8	582.5	626.3	637.4	678.3	587.5
VII	7	543.3	574.9	677.8	816.8	853.0	881.1	724.5

Averages per boy per test calculated from the individual figures taken to the nearest ten:

	48.5	51.7	58.5	62.4	63.6	69.5
Standard deviation	6.8	8.1	9.9	14.0	15.4	15.2

It is interesting to note that even in this simple exercise the boys in the upper classes draw further and further away from the lower ones as the tests proceed.

The coefficient of variability (found by dividing the standard deviations by the averages), which commences approximately at $\frac{1}{3}$, rises to $\frac{1}{2}$ and then to $\frac{1}{4}$ as the exercises continue. It will be remembered that all these boys are eleven-year-old children and are in no way selected, since every boy in the school who was eleven at the date of the tests worked the exercises. I called attention to this feature in the work of school-children¹ when discussing the educational question of the same curriculum for all. Such a curriculum may not produce equality; often, on the contrary, an increased divergence may appear between children of varying abilities.

The correlations between the results of the successive exercises were worked out from the individual results by the product-moment formula of Pearson. The coefficients were found to be as follow: between the first and second, +.819; between the second and third, +.825; between the third and fourth, +.842; between the fourth and fifth, +.951; and between the fifth and sixth, +.909. The probable errors were negligible. This Test, carried out under the conditions indicated, is evidently of very high reliability.

¹ *Mind*, XVIII. No. 69: "A Modern Basis for Educational Theory," by W. H. Winch.

The following is a brief summary of the results of the six Complex Motor Tests :

Complex Motor Tests. Average Marks per Boy per Test.

School Standard	No. of Boys	Wed. p.m.	Friday p.m.	Wed. p.m.	Friday p.m.	Wed. p.m.	Friday p.m.	Average of Six Tests
III	1	65.0	64.0	97.0	91.0	102.0	136.0	92.5
IV	3	107.7	127.7	144.7	169.7	170.3	194.0	152.3
V	11	116.5	146.5	181.5	198.1	231.5	246.2	186.7
VI	19	124.1	149.8	176.9	196.2	218.1	239.3	184.1
VII	7	149.4	174.4	218.0	239.0	257.1	283.3	220.0

Averages per boy per test calculated from the individual figures taken to the nearest ten :

	12.4	15.0	18.1	20.0	22.2	24.2
Standard deviations	3.6	3.5	4.2	4.3	5.1	5.0

Contrary to what might have been expected *a priori*, the abler boys (as estimated by school progress) do not, in this more complex exercise, draw away from the boys in the lower classes as they did in the simple motor exercises; the coefficient of variability falls rather than rises as the exercises proceed. The reliability of the Test is very high. The coefficients of correlation were as follow: between the first and second, +.874; between the second and third, +.931; between the third and fourth, +.951; between the fourth and fifth, +.952; and between the fifth and sixth, +.930. The probable errors were negligible. The coefficients were, as before, worked out from the individual results by means of the Pearson product-moment formula. It would appear, therefore, that this test, as well as the Simple Motor Test, if carried out under proper experimental conditions, is very reliable and gives most steady results. It will, I think, scarcely be doubted that, in so far as these tests do yield a measure of motor functions, the number, the succession, and the steadiness of the tests are such that we may be fairly satisfied that we are not dealing with 'chance' results, but that the boys were working on their 'true form'."]

§ 5. I have independently calculated the values given in the preceding section, together with many others, and get results in some cases slightly different owing to the fact that I omitted from all my lists the single boy in standard III because he was so exceptionally inferior in every respect. The differences are hardly appreciable, but I refer to them here in order to avoid possible misunderstanding later on. Calling the two abilities tested x and y , I get the following sequence of correlations:

$$r_{x_1y_1} = .29$$

$$r_{x_2y_2} = .44$$

$$r_{x_3y_3} = .59$$

$$r_{x_4y_4} = .48$$

$$r_{x_5y_5} = .47$$

$$r_{x_6y_6} = .50.$$

Thus the general tendency is for the correlation coefficients to increase at first and then remain fairly steady. This is in conflict with the results of Binet¹ and Burt², and negatives the view that correlation is due simply to voluntary attention or, what is almost the same thing, to lack of practice.

Other correlations that throw light upon the factors at work are the following:

$r_{x_1y_2} = .32$	$r_{x_5y_6} = .51$	$r_{x_5y_4} = .51$
$r_{x_2y_1} = .27$	$r_{x_6y_5} = .51$	$r_{x_4y_5} = .40$
$r_{x_3y_4} = .63$	$r_{x_3y_5} = .60$	$r_{x_6-x_1} = .28$
$r_{x_4y_3} = .48$	$r_{x_5y_3} = .53$	y_6-y_1

The first two of these coefficients show that y_1 is less reliable than the other series. The last shows that improvement in the one test is not very highly correlated with improvement in the other.

The correlations between improvement and average ability in each test is fairly high,

$$r_{x(x_6-x_1)} = .78, \quad r_{y(y_6-y_1)} = .50.$$

§ 6. The correlation between the totals of the two tests is

$$r_{xy} = \underline{.56 \pm .07}.$$

We have now to consider whether it is possible to improve on this by means of Spearman's formula.

Taking measurements 1, 3 and 5 together for group a , and 2, 4 and 6 for group b , we should have

$$S(x_a y_a) = S(xy) = S(x_b y_b).$$

On calculation (totals divided by 10),

$$S(x_a y_a) = 6099, \quad S(x_b y_b) = 8079.$$

¹ *L'Année Psychologique*, 1899, vi. 395.

² This *Journal*, 1909, iii. 168. It should, however, be noted that Burt is considering the successive correlations of different tests with *intelligence*, so that our results are not necessarily incompatible with his, especially if our correlations are to be considered as mainly due to a community of *specific* rather than general factors.

$$\text{Now P.E. of } S(xy) = \frac{.67449}{\sqrt{n}} \sqrt{\frac{S(xy)^2}{n} - \frac{S(x^2) S(y^2)}{n^2}},$$

$$\therefore \text{ P.E. of } S(x_a y_a) = 97, \quad \text{P.E. of } S(x_b y_b) = 131,$$

$$\therefore \text{ P.E. of } S(x_b y_b) - S(x_a y_a) = \sqrt{97^2 + 131^2} = 163.$$

Since this value is less than $\frac{1}{3}(8079 - 6099)$, Spearman's formula is inapplicable.

(Had we applied the formula, the corrected value would have been '61.)

Again, taking 2 and 5 as group *a*, and 3 and 4 as group *b*, we have (totals divided by 10)

$$S(x_a y_a) = 3494 \pm 56, \quad S(x_b y_b) = 3834 \pm 62.$$

P.E. of difference = 83, which $< \frac{1}{3}(3834 - 3494)$, thus again contradicting the assumptions of the formula.

In this case, also, we find

$$r_{\frac{x_a - x_b}{y_a - y_b}} = .13 \pm .10 \text{ instead of } 0.$$

Finally, taking 1 and 3 for group *a*, and 2 for group *b*, we have

$$S(x_a y_a) = 48554 \pm 821, \quad S(x_b y_b) = 45740 \pm 774.$$

$$\text{Difference} = 2814. \quad \text{P.E. of difference} = 1128.$$

Here the difference is nearly three times its P.E., thus excluding the use of the formula.

The conclusion to be drawn from these empirical tests is that either the various methods of grouping (which Spearman himself recommends) have not effected the elimination of the 'regular' deviations, or the residual 'accidental' deviations are not uncorrelated with one another or with the abilities themselves, or that both alternatives are realised. One cannot help feeling that the term 'accidental' is here used simply as a cloak for ignorance and that no assumptions as to the correlation or non-correlation of such deviations are in the least justified. Our attitude in this as in so many problems of statistical psychology should be one of extreme empiricism. There is still the big problem of individual variability and its correlations to be reckoned with, as I pointed out three years ago. Unfortunately six tests are not sufficient to give any satisfactory measure of this, so that the problem must stand over for a later research¹.

¹ For the sake of completeness, however, I have worked out the correlations of variability and ability on the basis of these six tests, and get the values:

$$r_{\frac{x}{\text{M.V. of } x}} = .74; \quad r_{\frac{y}{\text{M.V. of } y}} = .47; \quad r_{\frac{\text{M.V. of } x}{\text{M.V. of } y}} = .16.$$

The following table of means, standard deviations, and coefficients of variation $\left(\frac{100\sigma}{\text{mean}}\right)$ throws additional light upon the matter of the 'regular' deviations.

	x_1	x_2	x_3	x_4	x_5	x_6
Mean	487	520	581	653	667	701
σ	70.6	79.2	82.4	136.0	146.5	145.4
$\frac{100\sigma}{\text{Mean}}$ (coefficient of variation)	14.5	15.2	14.2	20.8	22.0	20.7

	y_1	y_2	y_3	y_4	y_5	y_6
Mean	125	152	183	202	225	246
σ	35.1	32.4	40.0	40.9	47.2	48.0
$\frac{100\sigma}{\text{Mean}}$ (coefficient of variation)	28.0	21.4	21.8	20.2	21.0	19.6

The shifting of the mean is slightly non-linear in both cases, though more definitely so in the case of the x 's. The coefficient of variation of the x 's increases from the first half to the second half of the series while that of the y 's remains fairly constant after the first. There is a marked increase of steadiness of the figures towards the end of the series in both cases. The conclusion to be drawn is, I think, that for the accurate determination of a correlation coefficient a large number of measures should be made at fixed intervals throughout an extended period of observation, the means and σ 's being determined in each case in order to serve as controls, and then the later measurements showing a sufficient degree of constancy of mean and σ should be averaged and the coefficient calculated from them alone. This method would eliminate 'observational errors' as completely and safely as possible, since the 'accidental,' as distinct from the 'residual' deviations would be approximately neutralised by the law of averages, and their correlations, even if an actuality, would be eliminated with them, while the 'residual' deviations themselves would be reduced to a minimum. It is of course

free from the criticism that the 'uncorrected' coefficient contains the same disturbances that render the use of the formula invalid¹.

In the present case we must content ourselves with the mean of all six measurements, since the number is too small to allow of the neglect of the earlier ones.

§ 7. The question as to what exactly causes correlation between measures of different forms of mental ability is a very difficult one, and can only be adequately considered on the basis of results from that 'intensive' method of research to which I have already referred as the research of the future. The causes may—some of them certainly *must*—come from without rather than from within. Mechanical memory correlates with power of logical reasoning in all probability because in the struggle for existence and attempted adaptation to one's environment the former ability was a precondition of the development of the latter. The school environment, viz. the methods of teaching, may in like manner contribute to the production of a correlation between mathematical ability and scientific ability (in chemistry and physics). The inner factors are partly general, partly specific, and the interesting article of Hart and Spearman² is an important contribution to the study of those that are general. The material they work with certainly indicates the probability of the existence of a general source of correlation, although the absence of probable errors in the correlations of such short series of correlations as those with which they had to deal deprives their conclusions of the quality of absolute mathematical demonstration. So far as my own results were employed in this investigation, I still think that my own more cautious conclusions were all that could be safely drawn from my data. With the exception of the tests that patently measure intellectual ability and cognate aptitudes, the correlation coefficients are so low as compared with their probable errors that they contribute very small weight to the 'correlation of parallel series of correlation coefficients' which is in this work taken as the fundamental

¹ Cf. C. Spearman, "Der Beobachtungsfehler in der Korrelationslehre," *Ztsch. f. ang. Psychol.* 1912, vi. S. 74. Spearman writes here: "Brown und Betz haben jedoch einen groben Fehlschluss begangen, indem sie daraus entnahmen, dass die Störungen dem Ergänzungsprozess zur Last fielen. Denn diese Störungen sind schon im gewöhnlichen unergänzten Koeffizienten enthalten." This is of course quite untrue of the 'accidental' disturbances, if, as I recommended, a sufficient number of independent measurements are made of which the mean is taken. The 'regular' deviations do remain in both and can best be reduced to a minimum by the procedure recommended above.

² B. Hart and C. Spearman, "General Ability, its Existence and Nature," this *Journal*, 1912, v. 51-79.

criterion of the existence of a 'central factor.' And in the more recent research of Mr Stanley Wyatt¹, which supports the theory of a central factor, the tests employed are in most cases chosen with special reference to their involving what we in ordinary parlance know as 'general intelligence.' There is also the danger, which I pointed out in one of my own researches some years ago, that community of external influence, heterogeneity of material, and other 'irrelevant' factors may superimpose 'spurious' correlation upon the results, thus emphasizing the general causes of correlation as compared with the specific.

If the sole general cause of correlation is a certain 'common fund of energy,' the order of closeness of correlation of different tests should be constant in different groups of subjects, as I pointed out in 1910. Results hitherto obtained do not show this agreement, and we can only hope for conclusive evidence on the subject from a more accurate determination of the individual correlation coefficients.

¹ Stanley Wyatt, "The Quantitative Investigation of Higher Mental Processes," this *Journal*, 1913, vi. 109-133. The chief reason why he obtains larger coefficients than I did for the same mental abilities is that his groups of subjects were not so stringently selected as mine were. Selection always reduces correlation. I now think that, for this reason, my own correlations were too low, although freer from the danger of superposed spurious correlation than they would otherwise have been. As soon as one mixes classes, even of children of the same age and school, spurious correlation, due to difference of discipline, etc., comes into play.

[*Manuscript received 20 August, 1913.*]

APPENDIX I.

CORRELATION TABLE ($N=500$). Showing correlation between lengths of lines and errors made in reproducing them.

Errors made, in mm.														
23-21	21-19	19-17	17-15	15-13	13-11	11-9	9-7	7-5	5-3	3-1	0 -1 +1	+	+	
30-35	—	—	—	—	—	—	1	2	1	6.5	13.5	2	1	
35-40	—	—	—	—	—	—	.5	.5	2.5	3	11	6	7	
40-45	—	—	—	—	—	—	—	3.5	4.5	3.5	7.25	9.25	3.75	
45-50	—	—	—	—	—	—	—	4	4	2	5.25	6.25	5.75	
50-55	—	—	—	—	—	1	—	—	4	2.5	7	2	8.5	
55-60	—	—	—	—	—	—	—	3	5.5	5.5	2	4	4.5	
60-65	—	—	—	—	—	—	—	2	5	5.5	5.5	7	7	
65-70	—	—	—	—	—	—	1	—	3.5	2.5	7.5	4.5	6.5	
70-75	—	—	1	1.5	—	1	4	3.5	3.5	2	3.75	6.25	5	
75-80	—	—	—	.5	—	—	1	2.5	1.5	7.5	2	3	4	
80-85	—	—	—	—	—	2	2.5	7.5	2	7	8.25	4.25	4.5	
85-90	—	—	—	—	1.5	3	3	2	—	5.5	4.75	4.75	8.5	
90-95	—	—	—	1	.5	1	2.5	1.5	6	3.5	5	4	1.5	
95-100	—	1	—	1	1	—	1	.5	.5	4	5	4	—	
Totals ($\sum x$)	1	1	1	4	3	8	16.5	32.5	43.5	60.5	96	67.5	72.5	
Means (\bar{x})	+7	+4	+2	+4.375	+5.833	+3.625	+3.091	+7.23	+0.11	+8.10	-.305	+204	+507	

Length of lines, in mm.	+	+
30-35	1	3-5
35-40	7	3.75
40-45	3	9.25
45-50	2	5.75
50-55	8.5	6.25
55-60	4.5	2
60-65	7	4
65-70	4.5	7
70-75	5	4.5
75-80	5	6.25
80-85	5	4.25
85-90	4	3
90-95	4.5	4.25
95-100	1.5	4.75

$$\sigma_x = 2.701 \quad S(xy) = 173.34, \therefore r = \frac{173.34}{500 \times 2.701 \times 3.896} = .033 \pm .0801.$$

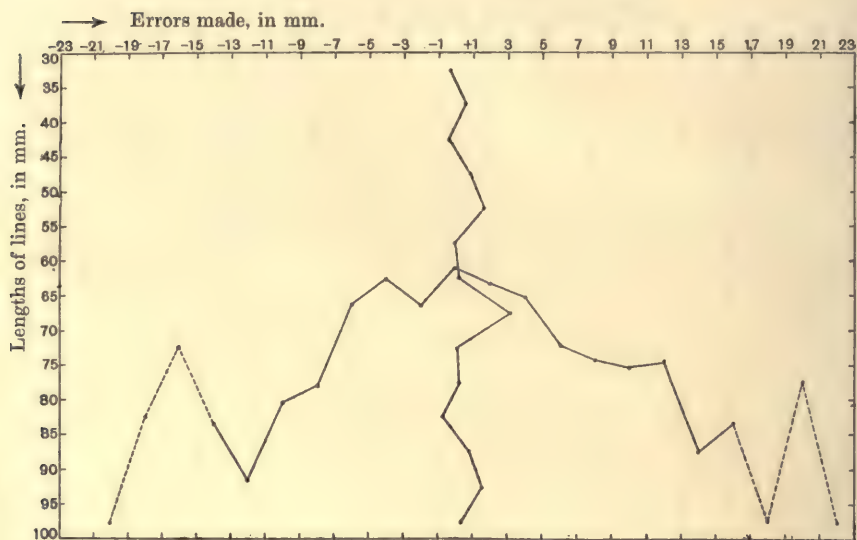
5-7	7-9	9-11	11-13	13-15	15-17	17-19	19-21	21-23	Totals (n_y)	Means (\bar{x}_y)
1	1	—	—	—	—	—	—	—	29	—
1-25	—	—	—	—	—	—	—	—	30	—
2-25	3	—	—	—	—	—	—	—	33.5	—
2	1.5	—	—	—	—	—	—	—	32.5	—
2.5	2	1.5	—	—	—	—	—	—	30	—
2	2	1.25	.25	—	—	—	—	—	27.5	—
6.5	1	2	.25	—	—	—	—	—	37.5	—
2.5	3.5	2	2	—	1	—	—	—	38	—
2.5	2.5	2	1	—	—	—	—	—	44.5	—
5.5	.5	2	—	—	—	—	1	—	42.5	—
5	4	2.5	.5	1	—	—	—	—	40	—
2	5.5	1.5	1	—	1	—	—	—	45	—
2.5	1	—	—	—	—	1	—	—	45	—
								1	25	—
41.5	25.5	14	5	1	3	1	1	1	$N = 500$	$\bar{x} = .352$ $\sigma_x^2 = 7.297$
+1.910	+2.383	+2.518	+2.450	+5	+3.667	+7	+3	+7	$\bar{y} = .883$ $\sigma_y^2 = 15.177$	

Arbitrary Mean

$$\eta_y^2 = \frac{S\{n_x(\bar{y}_x - \bar{y})^2\}}{N\sigma_y^2} = .104, \quad \therefore \eta_y = .323 \pm .027.$$

$$\eta_x^2 = \frac{S\{n_y(\bar{x}_y - \bar{x})^2\}}{N\sigma_x^2} = .0332, \quad \therefore \eta_x = .182 \pm .029.$$

APPENDIX II.

Regression Curves of Correlation Table.

THE MAIN PRINCIPLES OF SENSORY INTEGRATION¹.

By HENRY J. WATT.

- A. 1. *The systematization of the sensations.*
 - a. *Note on extensity.*
- 2. *The systematization of the integrative sensory modes.*
 - a. *Note on the word 'mode.'*
 - b. *Note on the word 'integration.'*
- B. *The main principles of integration.*
 - 1. *"The mode which results from the integration of an attribute must bear an immediate introspective resemblance to it."*
 - 2. *"The results of the integration of the same generic attribute in the different senses must be introspectively and functionally similar."*
 - a. *The sub-principle of the explanation of apparent exceptions and limitations to this rule: "If a mode of experience does not occur where we might for any reason expect it, this can be explained only by the absence of the variant experiences upon which it is integratively dependent and for the latter the natural limitations of physical and physiological processes must be ultimately responsible."*
 - 3. *"Every typical mode of experience must to some extent at least arise spontaneously and automatically and independently of such processes as will, attention, inference, proof, or the like."*
- C. *Conclusion.*

THE formulation of principles is an important stage in the advance of any science. Its beneficial effects far outweigh its disadvantages and

¹ An abstract of this paper was read before the Sub-section (to Section I) of Psychology at the Meeting of the British Association at Birmingham, 1913.

dangers. It is the sign of an increasing unanimity, a concentration of criticism in various fields round one or two points of view, a growing sense of the inherent connexions of the subject-matter. It means the abandonment of extraneous principles of explanation most successful, it may be, in objectively neighbouring provinces of science, but really inapplicable to the one under consideration. It serves, moreover, as a guide to research and to theory, thus supplementing mere exhaustiveness by some degree of enlightenment. And it is perfectly safe, unless it is the outcome of a movement towards prejudice and bias.

The formulation of principles is highly necessary in psychology, for it is recognised by many to be a sphere in which the effects of the interaction of all the main forms of being—physical, physiological, biological, psychical, and social—are made patent. The introduction of extraneous principles of explanation is highly probable in this case, unless sufficient attention be given to the nature and applicability of the principles to be admitted. The principles of the natural and biological world do, of course, make themselves felt in the sphere of experience. But they do not provide a sufficient basis for the proper systematization of that sphere. The peculiar nature of the psychical itself must be emphasized and principles must be devised for its elucidation which are drawn from its own sources and may therefore be expected to do the only full justice to its particular difficulties. This claim is, in fact, an assertion of the priority of the psychical in the psychical realm. It is also an assertion of the possibility and necessity of a purely psychological systematization of the psychical.

A. A systematic psychology of sensory experience is perhaps the greatest need of our science at the present time. It has been very much neglected. That, no doubt, is due to the fact that the chief motive of the study of the senses has been physiological. There seemed to be so much to be gained by this physiological study and so little air to breathe in a purely psychological atmosphere. But surely there is no use in talking of a science of psychology at all, unless the realm of sensory experience can be properly systematized. The simplest and most fundamental problems involved in this task fall into two main groups.

1. *The systematization of the sensations* is the first of these. Some sort of a 'periodic table' of the sensations must be formed, which will serve as a framework and basis for any theory regarding the qualities of sensation; and the attributes of sensation must be reduced to a type. This psychological task is a necessary preliminary to any pure psychology

of the senses. I have attempted to fulfil it elsewhere¹. Only a short summary and revision of the outcome of that attempt need here be given. Of the six attributes of sensation, quality and intensity stand somewhat apart from the others. Quality may be considered to occur only in single and discrete forms in all cases, except in the senses of vision and smell whose purely psychological treatment is still problematical. Hardly in any case is there any dispute or difficulty concerning intensity. The four other attributes—of extensity, order, duration, and position in time—may be arranged usefully in the following scheme:

Generic names of the various dimensions of sensation	Generic names of the attributes :	
	Extensity	Order
	These are inherent	
	WITHOUT	WITH
	variation, in all sensory	
(Intra-) Systemic	extents, masses, volumes	localisations, positions, itches
Temporal	durations	positions-in-time

It is tempting to bring quality and intensity into parallel with this scheme, so as to reduce the six attributes of sensation to a triad of pairs, each pair being extensive and ordinal in its own peculiar dimension. But this is impossible for the following reasons²:

(1) Qualities cannot be treated as orders, for they give no distances or motions; even if that fact be ignored, it is introspectively evident that they do not bear the stamp of an ordinal attribute. Even the different colours we do not think of as points in a system; how much less then do we consider the qualities of the different senses in this

¹ "The Elements of Experience and their Integration; or Modalism," this *Journal*, 1911, iv. 135 ff., esp. 148 ff. *Psychology*, London, T. C. and E. C. Jack, 1913, 21 f. Cf. "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 f.

² Cf. my paper "Are the intensity differences of sensations quantitative?" This *Journal*, 1913, vi. 176 f.

242 *The Main Principles of Sensory Integration*

way. And if quality is not an ordinal attribute, it is certainly not a merely extensive attribute.

(2) Intensity, likewise, can be treated, neither as an extensive attribute, for it is essentially variable and is not introspectively identifiable with the extensive form of attribute; nor as an ordinal attribute, for it is neither phenomenally nor functionally like one of these.

(3) If quality and intensity formed such a pair of attributes, they should prove readily adaptable to quantitative purposes, as do the attributes of extensity and order in combination with one another in connexion with the measurement of space and time. But this is not the case.

a. Note on Extensity. The critical point of any discussion of this attribute lies in the problem of its relation to the attribute of order. When extensity is present in a pure form, according to Stout, as in the case of the voluminousness of sounds, "it has no distinctively spatial character, no internal order of positions and distances¹." It seems as if the quantitative aspect of space could exist without a spatial order². Such statements suggest the following question, which may be expressed in various forms: Is extensity as an attribute really variable? Has it for example, a minimum, say the sensory 'spot'? Or we might ask: Is the extensity of the minimum different in variety or amount from that of a postage stamp? Is the voluminosity of a high tone different in variety or amount from that of a low tone? Surely it must seem absurd to suggest assent to these questions.

What, then, are we to understand by the differences referred to, *e.g.* the 'vast discomfort of a colic or lumbago,' the peculiarities of high tones and of low tones, the differences of the areas felt from the contact of a pencil point and of a postage stamp? If extensity and massiveness and voluminosity do not differ, extents and masses and volumes surely do; these are the things we distinguish in these cases. But obviously no part is played in their composition by quality or by intensity, not to mention the temporal attributes. The only other attribute besides these and extensity is order, which does vary.

We may, therefore, suppose that extents and masses and volumes of sensation differ in virtue of the varying number of orders included within them (or by the varying number of sense-organs of neighbouring

¹ G. F. Stout, *Manual of Psychology*, 1899, 337.

² *Op. cit.* p. 334. Cf. also p. 336: "We have all kinds of gradations between pure extensity and fully definite extension." "Typical cases of extensive diffuseness or massiveness are afforded by organic sensations" (p. 337).

position that have been excited). This conclusion is quite consistent with the psychology and the physiology of the cutaneous, gustatory, and visual sensations. Hesitation can only arise in connexion with the massive sensations, articular, muscular, organic, and auditory. But it must yield to a reiteration of the priority of psychological systematization and of the probable conformity of the results of physiological study thereto. If muscular sensations from muscles of different size, and articular sensations from joints of different size, differ in massiveness, surely there need be no hesitation in supposing that this difference is correlated with a difference in the number of receptors excited. The same remark applies to the sensations of colic, lumbago, hunger, thirst, and the like. The varying voluminosity of sounds suggests that each sound is really a mass or extent of sounds; high tones are thin and short, low tones are longer and perhaps bulkier, and, it may be, more tenuous as well. Such a view would explain why the pitch and the voluminosity of tones are fixedly correlated with one another. It is the psychological statement to which Ewald's theory of hearing¹ in many respects forms a most suitable physiological counterpart.

But although extensity is not variable, it is a true attribute of sensation, readily distinguishable from order. Without it we should have neither areas nor voluminosities. That is evident if we remember that a cognitive form of order² exists to which there is no accompanying extensity, so that it is impossible to make a series of concepts, such as those of number, adequately represent the real continuity of an objective line or area. It might be supposed to be a sort of sensory stuff, which is repeated and multiplied by the repetition of orders. But the same notion would apply equally to any of the other attributes. The quantitative treatment of extents and durations is possible, only in virtue of the close, psychical kinship between sensory orders and conceptual orders; in a certain respect the latter grow immediately out of the former, although they are extended very much beyond the range of the variations of sensory order. Measured extents are not measured extensities at all; for, as we have seen, extensity is not varied. But extensity can be involved indifferently in a statement of what is measured, because it is itself unvaried and can, therefore, introduce no confusion or complication into the comprehension of that statement. Extensity, for the same reason, seems to have a minimum only in relation to order. A distinction of orders within the 'spot' is, of

¹ J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 147 ff.

² Cf. K. Bühler, *Arch. f. d. ges. Psychol.* 1907, IX. 357 f.

244 *The Main Principles of Sensory Integration*

course, thinkable, but it does not exist in sensation. So extensity seems to be variable only in conjunction with orders, especially when the latter are all continuously adjacent and are given along with uniform quality and intensity. Then the fusional function of extensity comes into action and we get continuous extent or area. But the differing orders involved in this extent, though no longer separately distinguishable, are effectively present. It is just they which determine the extent of the sensational area or mass.

If orders are to be separately distinguishable under areal or massive conditions, they must evidently be accompanied by variation in some other attribute. The only other variable attributes are quality, intensity, and position in time; but there may be variation in more than one of these at the same time, of course. This consideration seems to be of some importance for the theory of orders and their complications¹.

It must be obvious that the above statements apply equally to the attribute of duration. It is essentially an unvaried attribute, which gives variable durations or stretches of time only in conjunction with the variable attribute of position in time.

2. *The systematization of the integrative modes* of sensory experience is the task that for a scientific psychology inevitably follows upon the systematization of the simplest sensations. In so far as these modes occur under different circumstances, they must be identified and reduced to types of graded complexity and referred to their typical conditions, so as to come within the purview of a general, systematic theory of the constitution and interconnexions of experiences. Of these modes there are two main groups—those which take place between sensations which belong essentially to the same sensory system and those which take place between sensory experiences which, like those of the two eyes or the two ears, belong to different systems. Of the former, intrasystemic integrations, distance, and interval of time are the simplest. In many cases they involve a difference in the sensations which make up the distance or the interval of time only in respect of the attribute of order or of position in time; and in those cases in which a variation in extent or in duration is noticeable without any accompanying discreteness or separateness of sensations in respect of order or of position in time, we are justified by consideration of the circumstances of stimulation in extending our statement and in assuming that, in these cases also, distance and interval of time are based upon sensations which differ only in respect of the attribute

¹ Cf. my discussion of "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 ff.

of order or of position in time. Moreover, distance occurs only in those senses whose sensations differ readily and obviously in the attribute of order. We are never called upon to distinguish hunger or thirst distances, or distances of muscular sensation, or smell distances. In these senses the variation that we notice is at most one of extent or of massiveness. In so far as distance occurs in different senses, however, we must expect and do find that it is phenomenally and functionally the same.

All experiences are qualified by position in time of some form; consequently we can experience an interval of time between any two experiences. But the interval is distincter when it is constituted by experiences belonging to the same sense, and still more so when it is given in those senses which are specially rhythmical, namely sound, vision, and the motor group of senses—the articular, the muscular, and the tactual. In these senses the stimulus can be readily manipulated so as to cause an experience to begin and to cease at any desired moment.

Distance and interval of time are, as modes of sensory experience, peculiarly simple, in that they are the only modes which necessarily involve a variation in only one of the attributes of the sensations upon which they are, or may legitimately be supposed to be, dependent. On the other hand, order and position in time are themselves the only two attributes of sensation which can vary apart from variation of any of the other attributes of sensation. Thus analysis confirms the introspective simplicity of these modes.

The sensory mode that stands next to these two in point of simplicity is motion. For many reasons it may be considered to be a combination of the modes of distance and of interval of time. It is therefore found in those senses which present the mode of distance. Its phenomenal and functional identity in these senses, especially in that of sound where it forms a part of what is collectively called melody, is a problem of great interest at the present time. But the study of motion presents peculiar difficulties¹. For the present it may suffice to say that motion is a combination of the two modes of distance and of interval of time, involving simultaneous and continuous, though not necessarily concomitant, variations in the attributes of order and of position in time of the sensations which integrate to form it².

¹ Cf. "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 ff.

² For preliminary work towards the systematization of the modes of distance and of motion, see my paper in this *Journal*, iv. 172 ff. and 157 ff.

246 *The Main Principles of Sensory Integration*

a. *Note on the word 'mode.'* I find the use of this word very convenient¹. It serves, of course, in the first place to distinguish those experiences which we may legitimately suppose to be integrated out of simpler experiences, from experiences such as the simplest sensations which show no sign of such derivation. But if we may presuppose the systematic classification of these modes, we can then with the help of this word and of adjectives signifying the name of each class of modes indicate without any ambiguity or confusion exactly the kind or complexity of experience involved in any particular state of mind. That cannot be done with the commonly used word 'perception.' When we speak of the perception of distance, it is not clear what exactly is meant. Do we mean the perception of distance as an object for the mind or as an experience, or do we merely mean the presence and effectiveness of distance in our sensory experience? If we wish to study perception as distinct from any sensation or sensory mode, we can indicate that by speaking of the study of the perceptual modes of experience.

The word 'mode' will also translate the German word *Vorstellung* in many of its uses, for example in its application to the term *Gestalt*, which has been used to indicate distance and motion and many other experiences which differ from sensation in the same way as these do. But it can only be misleading to talk of the 'quality' of a mode or *Gestalt*. Every mode has its own introspective nature and affinities, but these have only seldom anything to do with quality. Although the unqualified use of the word 'mode' well translates the unqualified use of the word *Vorstellung*, the use of the latter word is apt to be as misleading as the English word perception, e.g. when we read in one sentence of the *Vorstellung der Zahl*, *Vorstellung der Distanz*, *Vorstellung der Aehnlichkeit*, and *Vorstellung der Verschiedenheit*². There are such things as sensory number and difference, but they are surely not modes, the same things as are distance and motion; there is a sensory mode of distance and a conceptual mode of distance, but there is a great difference between them. We proceed unscientifically if we lose sight of these differences.

b. *Note on the word 'integration.'* This word indicates that the resulting mode unifies the sensations to which it refers and is attached and upon which it is psychically, if not also psycho-physically, dependent. The word may therefore be used generally to express the known relations

¹ Cf. this *Journal*, iv. 203; *Psychology*, 1913, chaps. II. IV.

² E.g. Witasek, *Grundlinien der Psychologie*, 1908, 222 ff.

between modes of experience and the simpler experiences upon which they rest. And an inductive study of these relations in various cases may be expected to lead us on to knowledge we could not gather from any one particular case. So the word integration may imply the general theory of the relation of a mode to its basis in experience, which psychology may hope some day to attain. If this is borne in mind, the use of the word can make neither for obscurity nor for confusion, but can only be the means of scientific concentration and inquiry.

B. After these preliminary statements we may now consider the main principles of sensory integration.

1. The first principle is as follows: *The mode which results from the integration of an attribute must bear an immediate introspective resemblance to it*¹. Or: Among the attributes or features of the simpler experiences upon which a mode of experience is, or may legitimately be supposed to be, psychically dependent, there must be one to which it bears a much greater introspective resemblance or affinity than to any other. The latter statement is more inductive in outlook, while the former is more deductive. Only on the basis of such a principle as this can a theory of psychical derivation or causality be built up which will reveal in the world of mind that rationality and intelligibility which we naturally expect to find in all things. The position involved in this principle has been reached by psychology in three distinct steps.

a. For each variation in the derived or integrated state analysis and experiment must show an unambiguous complex of stimulatory or sensory data. This is an obvious and uncontestable truth. Only about the relation of the derived state to the experiences with which it is objectively correlated can there be any dispute.

b. Either: we talk in all cases only of stimulatory data, no matter what the experiences we are investigating may be, mere aggregations or unique modes. This position is taken by very many psychologists of the present time. It leaves, of course, no room for the principle stated above; but neither does it leave any room for a science of pure psychology. All we can then expect is a mere distinction of mental states from one another and a correlation of them with *physical or physiological data, that is to say, psycho-physics or psycho-physiology*. An inquirer of a logical turn of mind might well ask how we can have mere distinction without some trace of interconnexion by resemblance, and, thereafter, without some theory in explanation of this resemblance; but if this thought arises in the minds of those who remain at the

¹ Cf. my *Psychology*, p. 26.

position of this paragraph, it is rendered ineffective by some indefinite belief which makes any hope of constructing a reasonable explanation of the merely similar, or generally of the psychical, untenable. It must, of course, be obvious that if there can be no pure psychology of sensory experience, there can be no pure psychology of any kind of experience at all.

Or: we allow a resultance of certain experiences from others by association or by 'experience,' while denying the principle under discussion. This position is closely associated with the theory of local signs, but it is also in vogue with many in the treatment of cognitive and other experiences. But it must be clear that the effect of experience is unintelligible and association is impossible unless each of the associating elements already differs from every other, whether it be by its locality or order, or by its place in experience, or what not. A series of identicals cannot be differentiated by any association with a series of variants, if that association operates from the identical elements towards the variants. To allow this would be to deny the truth of the rule stated under (a) above. This alternative position, then, allows of a pure psychology, in the sense of a system of correlations of an objective kind between single experiences or between groups of experiences. But it blocks the prospect of an intelligible and reasonable science of experience. We must look for a corrective to its negative attitude in further insight into the origin and nature of association.

c. Association cannot be mere blind mechanism, a sort of bond that arises when experiences impinge upon one another in the mind and that requires no sort of counterpart or basis of origin in the experiences that become associated. The purely mechanical view of association prevails at the present time in the treatment of memory; for association can be treated systematically from a mechanical point of view. But this abstract theoretical procedure may be only a part of the whole truth. Purely mechanical memory involves the assumption that experiences associate when they come into contact in the mind in complete indifference to the affinity or dissimilarity of their 'contents.' The most reasonable constellation of ideas, then, has a greater coherence than any other grouping only because there are in it a greater number of frequently repeated and therefore strong associations. Meaning is just a general convergence of associations. But this is surely not confirmed by the facts. What is associated must surely cohere as conscious experience before the association arises. Of course

there must first be contiguity of a certain degree between the associating parts; they must occur within a certain stretch of time. But must we not suppose that having thus occurred they cohere because of their psychical affinity, and that having cohered and integrated they can then become associated to one another so that the one can revive the other? Mere mechanical memory means mental chaos and irrationality. Fortuitous contiguity would as easily produce a coherent mind, as fortuitous grouping of elements and natural selection would produce the biological world without the coherent basis of law given in the physical and chemical world. "A unitary mode of experience in which the associating experiences are integrated is always presupposed, although it is usually ignored¹."

This principle is the outcome of all unsuccessful attempts to derive special experiences from the grouping of other kinds of experience with the help of association alone. Neither local sign, nor stereoscopic vision, nor perception, nor the concept, nor recognition, nor thought, nor any other unique and special kind of experience, can be satisfactorily explained in this way. And if we must return to a direct consideration of the basis of coherence or of integration in the introspective nature of the experiences that form the basis of integration in all these cases, must we not also look for an integrative basis in experience even in the case of the seemingly most mechanical of associations? We may be in doubt about thus generalising the result, but there can be no hesitation about accepting the principle in the case of all unique modes of experience. If the objective dependence of one experience upon others compels us to classify it as a special mode of experience, and if we may therefore hope for a theory of its derivation or integration out of some one or more features of the experiences it is psychically dependent upon, then it is clear that we can look for its integrative basis only among those features of the experiences upon which it is dependent which bear an introspective resemblance to the mode in question. The true basis of integration will bear a greater resemblance to the mode in question than any other feature of the integrating experiences. It is evident that such a principle will serve as a guide both to experimental research and to theory. Moreover, if a mode is variable, the components of its integrative basis must be variable, as in the cases of distance and feeling; but if it is invariable, as in the case of recognition, the components of its integrative basis cannot be variable.

Whatever is, is rational. In reference to the present position

¹ *Psychology*, p. 60. Cf. this *Journal*, iv. 130, 139, and esp. 149 f.

of integrative psychological theory, this means that if we are to suppose that dependent mental states are derived from the integration of those upon which they are dependent, it would seem to us more satisfactory and intelligible that there should be some degree, or the highest possible degree, of resemblance between the dependent state and the feature or attribute of the conditioning experiences upon which the former in the case of variable modes is known to be dependent and in the case of invariable modes may be supposed to be dependent. More than this we cannot expect. If unique types of experience do not bear quantitative relations to one another, the relations that exist between them cannot in all cases be those of the type of reasoning. For that would be a denial of their specific nature. A standard for the discovery of these relations can then be found only in some other general appeal which the typical form of these relations in known cases may make to our minds. One element in that appeal at least must be degree of resemblance between integrative basis and derived mode. What other elements it may contain inductive research will show. Only on these lines can we hope for a science of pure psychology.

2. The second principle of integration is as follows: *The results of the integration of the same generic attribute in the different senses must be introspectively and functionally similar*¹. Stated more generally it reads: the introspective and functional nature of an integrated mode of experience is essentially independent of the attributive or other accompaniments of its integrative basis. Wherever the requisite integrative basis occurs, the same generic mode will result. This principle is a necessary step in the systematization which is to constitute a pure psychological science. I have attempted to establish it in detail in the case of the simplest sensory modes of distance and motion². But it must also hold in such cases as feeling, recognition, thought, and the like, for these can be occasioned by the most varied sensory and other experiences. The integrative bases of any mode must be considered to be the same in all cases, no matter what the accompanying differences may be. Experimental research will undoubtedly lead to the confirmation of this principle in all accessible cases. Very often the similarities of modes are passed by as mere analogies. That may serve as a good maxim where there is no insight into the systematic nature of experience to act as a guide. But it would be wrong to block the outlook

¹ Cf. my *Psychology*, p. 27.

² See this *Journal*, iv. 157 ff.

and progress of systematization by an ascetic cult of this idea of analogy.

If this principle be granted, we can hope to establish general rules for the relation of generic modes to the generic attributes or features of the experiences from which they are integrated. For example, "motion is found developed upon every group of sensations which show distinct variations from one another in order¹"; and, "we find distance in all those senses which show order and are capable of the modification of motion²." Rules may also be expected to hold for the limits of time within which alone the integration of those modes that are based upon successive experiences can take place. For we have reason to believe that in so far as all experiences are qualified by the attribute of temporal order, all integrative processes which involve successive experiences are subject to certain limits of difference of temporal order.

This principle would also lead us to expect that if a certain mode of experience can be integrated from simultaneous components it should also result from the integration of components which follow one another within the time limits just mentioned. Conversely we should be able to transfer our expectation in a similar manner from successive to simultaneous integration of the same mode, unless, of course, differences in either of the temporal attributes be an essential part of the foundation of its integration, as is the case in the integration of motion. If the temporal attributes are not the essential basis of an integration, it is clear that any differences in them that fall within the time limits of integration, should be as irrelevant to the integration as is the presence of identical or unvaried attributes.

It cannot, of course, be evident in detail how far this irrelevance of accompanying differences, such, for example, as those of quality in the case of distance and motion, extends. But it is assured by a broad consideration of the conditions of occurrence of the various experiences hitherto distinguished by psychology. We must therefore be on the look-out for it; and if it is not forthcoming as we should expect, we must find good objective reasons for its absence. It is fortunate that in the finding of these good reasons we can accept the guidance of a minor principle of explanation.

a. *The sub-principle of the explanation of apparent exceptions to this law.* If a mode of experience does not occur where we might for any reason expect it, that can be explained only by the absence of the variant experiences upon which it is integratively dependent and for

¹ This *Journal*, IV. 157.

² *Ibid.* 173.

252 *The Main Principles of Sensory Integration*

this the natural limitations of physical and physiological processes must be ultimately responsible.

It is the task of science to expound with the utmost detail the nature of the coherence that binds events into unitary systems of greater and greater extent. Each particular science is concerned with a part of the whole that more or less obviously forms a unitary system. If it discovers in its sphere that kind of coherence that characterizes another sphere of science, it thereby joins with that other to form a system of greater extent than either. But it does not therefore identify its subject-matter wholly with that of the cognate science. The two remain distinct in so far as the forms of coherence that characterize them differ. Now no one would deny that the forms of coherence that characterize the psychical world differ very much from those that characterize the physical and the biological worlds. But they are not wholly independent; something is common to them all. For on any view whatsoever it is clear that our knowledge of the physical world is dependent, not only upon the actual occurrence of physical processes, but also upon the transmission of these in some form or other through the sense-organs to the central nervous system. We can know of a physical process only if the differences of the parts and the manner of the arrangement it involves can be brought into correlation with those involved in a unitary psychical process. This holds, not only for cognition, but also for any kind of adaptation that may exist between the physical and the psychical realms. Such adaptation can occur only in so far as by some means or other a correlation of process can be carried through the three kingdoms of the physical, the physiological, and the psychical. In so far as physical processes occur at a slower rate of change than the minimum required for psychical integration, we cannot become aware of them, unless we can secure some means of bringing their rate of change within the narrow compass of the mind. If a physical change cannot be made to affect a physiological organ appropriately, we must remain ignorant of it, unless we transfer it through some medium which we understand so as to obtain the appropriate effect. And so on.

The mode of distance, for example, cannot be produced apart from variation of the attribute of order; it is therefore practically absent from the organic, muscular, and olfactory senses. In the organic senses there may be a certain variation in massiveness, involving difference of orders, but we do not have a hunger distance or a thirst distance in any proper sense of the word. Similarly we notice that the muscular

sensations from different muscles differ in massiveness and are localised at different parts of the body, but the sensations that come from one and the same muscle do not seem to vary in massiveness or in localisation. Thus a muscular distance, which might be constituted by the simultaneous occurrence of sensations from different muscles can hardly occur without the simultaneous excitation of such tactual sensations as would form a tactual distance. The latter for various reasons, such as variation, frequency, and correlation with other senses and modes, have a cognitive value that the former can never acquire for want of variability. Muscular distance will therefore be so obscure or so blended with tactual distance as to be hardly noticeable. In the sense of smell, distance seems to be quite lacking. If there is any olfactory order or localisation it seems to be so unvaried as to be useless. And even if smell has its order in some other form than localisation, in us at least the sense is so sluggish that the variations of order necessary for distance cannot occur within the time limits of integration. The same reasons as prevent the occurrence of distance prevent *pari passu* the integration of motion.

Interval of time is found under all possible circumstances, in all regions of experience. Only in the form of rhythm is there any restriction to its occurrence. The reason for that fact has been already mentioned: only certain experiences can be made to begin and to cease at any desired moment or periodically. So we cannot have rhythms of taste, temperature, smell, organic sensation, feelings, ideas or thoughts.

The peculiar correlation which is found in the sense of sound between pitch and voluminosity is responsible for all the limitations of integration which specially characterize this sense. Pitch is an aspect of sound which represents the individuality of the sounding object much better than does its spatial localisation. Besides, it seems clear that if the latter had been maintained at all costs on the basis of simple sensation as a sort of local sign, the former would never have been developed. The greater advantage lay in the attainment of a discrimination of pitch even at the temporary or permanent sacrifice of a direct auditory form of localisation. But two more or less efficient methods of localisation have been secured—the mobile-ear-funnel method of many animals and the binaural method of man. As a consequence, however, of the preferential development of pitch we have no true experience of auditory solidity and the smaller variations of tonal interval are rendered highly unclear or even impossible by the presence of beats and intertones.

It is of interest in this connexion to recall a remark made by Ewald. He wrote¹: "Man begeht immer gewisse Fehler wenn man die Funktionsweise eines Sinnesorganes mit der eines anderen vergleicht." "Wenn der physikalische Anlass für eine bestimmte Empfindung sich in irgend welcher Weise ändert und dadurch eine Veränderung der Empfindung bewirkt, so scheint mir keine Uebereinstimmung im Wesen der beiden Veränderungen bestehen zu müssen." But this is a principle of apology which cannot be accepted from Ewald. For the merit of his theory, apart from its experimental foundation,—a merit that is brought forward into the light by his own sixth argument against Helmholtz's theory—is the facility with which the phylogenetic development of hearing can be traced with its help. For it is just because and in so far as the physical variants of sound have always been the same and the physiological apparatus they play upon has gradually changed in the course of the development of the race, that the psychical results have gradually developed. The peculiar nature of the physiological apparatus has secured for it, not a fragmentarily specialised development, but an equalised development. The system of sounds which results is just as equalised and balanced in its nature. Besides, Ewald does assume that there must be some agreement between physiological and psychical changes; for he postulates a special physiological means of getting round the necessity for this agreement in the case of the ear:—his coupled-buttons theory². This, however, is a forced and artificial way of overcoming his chief difficulty, which is to explain why, on his theory, we do not hear a series of identical tones for each component of a tone picture, instead of only one tone. In the light of his criticism of Helmholtz's theory, this part of Ewald's theory is just as fantastic as is Helmholtz's. For what *deus ex machina* is to make all these coupled-buttons-connexions for the organism? How are they to begin and to be progressively developed?

If we can once decide in what manner any mode of experience varies, we thereby obtain an index to the integrative basis of that mode. This guidance is of great importance in those cases in which the integrative basis of a mode stands in a complex psychical environment from which it is not easily distinguished or isolated. If the variation of a mode is restricted or if there is none at all, its integrative basis should consist of only one pair of unchangingly different experiences. Such a case may perhaps be exemplified by the mode of recognition.

¹ *Op. cit.* 181 f.

² *Op. cit.* 183 f.

A problem of considerable magnitude is presented in the case of the absence from certain minds of experiences known to other minds. Animals, for example, do not reason. Probably they also lack the general concept and all those cognitive experiences which involve it; they can hardly be supposed to localise their memorial experiences in their past. With all other simpler experiences they may well be presumed to be equipped. But if they can see and hear and smell and feel as well as we can, perhaps in varying ways better, why does their experience not develop upon this sensory basis to the heights it reaches in the human mind? The answer to be deduced from the principle here stated denies that the animal possesses the full integrative basis of the experiences it lacks. It would be presumptuous in the present state of knowledge regarding the higher cognitive states to attempt to indicate what is lacking or why it is lacking. An alternative view refers the limitation to restrictions set by the level of development that the brain of the animal has reached. But that explanation is either psychically blank and valueless, or it implies that a further development would add some experiences to those the animal already has and so make the appearance of the higher cognitive states possible. Thus either the view stated above is conceded, or it is assumed that the higher modes of experience come into being by direct dependence on the development of the brain, not through the medium of the simpler experiences of whose integration the modes in question may legitimately be supposed to be the result. On the alternative view a pure science of psychology is, of course, impossible. Such a conclusion can hardly be entertained seriously for long, whatever divergence of views there may be regarding the kind of elementary experiences that are lacking in the animal.

3. The third principle of integration is as follows: *Every typical mode of experience must to some extent at least arise spontaneously and automatically and independently of such processes as reason, thought, determining purpose, and the like, unless these processes themselves are the modes in question.*

If it be borne in mind that a mere aggregation of experiences presents no problem and that every mode of experience worthy of that name must make some new addition to experience, it might hardly seem necessary to state this principle explicitly. It might seem so obvious as to be trite. But much of the past and current theory of the growth and development of the mind so thoroughly ignores the problem of the unique modes of experience that the principle may seem

to contain a new and startling truth. There can be no universal guide to the development of the mind, be it called reason or thinking or self-realisation or teleology, or what not. The mind must develop when it can, when the conditions for that development have been given; and what then happens is really development, a step forwards, something new, no mere unmasking of the obscure. The only guide to mental development, if it can be properly called by that name, is the illumination each step of integration brings with itself. It is itself its own coherence and justification. It reveals its own necessity, in part at least, when it comes; but it cannot be foreseen. In the light of the preceding two principles, integrative processes are most reasonable and intelligible, and with increasing knowledge they will appear still more so. But they are not themselves the product of reasoning; they must arise spontaneously. It is important to emphasize this in view of the fact that thought and purposive determination and such other processes are not only the instruments of science, but are themselves also modes of experience which must arise spontaneously. As the instruments of science, reason and thought provide us with standards of coherence in the form of identity and repetition, approximation and similarity, and these are our favourite tests for the manifold forms of coherence we find in the various spheres of being, including the relations of modes to their integrative basis. But while retaining these tests even in these last cases, we must not lose sight of the fact that each unique integrative process is and remains unique, and therefore contains a justification of its own, which we can never hope to extract from it by any inductive or other cognitive procedure. That justification is simply the coherence and insight the integrative process itself is.

The higher cognitive and the conative processes bear another important relation to the integrative processes in that they may serve to extend the conditions under which they take place, to support them by making these conditions more enduring, more compatible with the limitations of integration, and therefore virtually wider in scope. Once an integrative process has occurred, its signs or criteria can be established for indirect use. "But unless our minds recognised, or thought, or felt spontaneously, we could never even begin to collect tests for the recurrence of experiences, or for the truth or falsehood of asserted relations or for the justification of beauty. Nothing but the direct insight of experience can set the mind the larger task of extending that insight to the uttermost bounds of reason¹."

¹ See my *Psychology*, p. 27.

In the case of certain *nova* of experience it is relatively easy to show that they are integrative modes, but it may be very difficult to show from what features of the experiences upon which they are, or may legitimately be supposed to be, dependent in an objective psychical sense they are integrated. This difficulty may be supposed to be due partly to the complexity of the experiences which regularly accompany the essential integrative basis, partly to the fact that the *nova* are *nova* and can draw the attention and be compared and generally be the basis of new integrative processes, as if they were independent elements. From another point of view, however, this peculiarity is of great advantage; for it maintains the same freedom of mind for all stages of development. As integrative processes are originally spontaneous, the mind can accept their product without making special reference by attention or otherwise even to those experiences that form the essential basis of the integration. We can compare distances, tonal intervals, motions and melodies, without troubling to compare the orders and times that constitute them. We are immediately aware of the identity or difference of the mode itself in the various instances given. Thus the subjective efforts of the mind can be applied to any level or to any one of all the integrative processes which arise spontaneously upon any given occasion. This statement is absolutely thorough-going, as we have already noticed that every integrative process, no matter what its nature, must, to some extent at least, be spontaneous and automatic. Effort and attention may have to be applied indirectly to procure its appearance, as when we adjust our sense-organs, our body, our actions, our memories, our thoughts, in order to maintain a certain stream of experiences. But that stream of experience must, to some extent at least, flow spontaneously. The attention may then be applied to any point of it, usually its highest, in order to aid the spontaneous integration which is taking place at that point. The aid given may consist in rendering the integrative basis stabler, or in reducing the differences which present themselves to within the limits of spontaneous integration by means of special manipulation of the corresponding stimuli, or in repeating the series of integrating experiences so that the binding power of associations derived from simpler forms of integration may extend the integration in question over a longer stretch of time than that natural to the integration. What cannot be brought simultaneously within the compass of the mind, so as to integrate spontaneously there, may be taken in successive series and made to pass through the mind so

rapidly that it will then spontaneously reveal all its integrative secrets.

C. CONCLUSION.

The first principle of integration is, by growing consent, almost agreed to already. In one form or another, sensory or motor, it is the only acceptable conclusion of the long-drawn-out discussion of the origin of local signs. They cannot be thought to originate out of the association or combination of anything that is not already local sign. What is derived is therefore not primitive local sign, but only the complications and modifications of local sign that arise under varying circumstances, on the basis of a correlation of the local signs of experiences of different systems, such as eyes, ears, vision and touch, touch and sound, vision and sound, etc. The same conclusion appears to be inevitable in the discussion of other important problems. The outcome of Jaensch's extensive investigation of depth is: "Die Tiefenwahrnehmung hängt aufs engste zusammen mit Wanderungen der optischen Aufmerksamkeit und den mit ihnen verknüpften Impulsen, also mit einer dem Gesichtssinn eigentümlichen Funktion. Hieraus erklärt sich, dass Tiefenwahrnehmung des Gesichtssinnes in keiner Weise mit Empfindungen und Vorstellungen, welche einem anderen Sinnesgebiet entstammen, identifiziert werden kann, sondern einem eben nur dem Gesichtssinn eigentümlichen Inhalt darstellt¹." A similar remark may be quoted from a discussion of the various theories that have been given for the state of recognition. In criticising Rabier's theory, Katzaroff says: "Pourquoi ces divers sentiments qu'invoque Rabier, sentiment d'absence d'effort et de nécessité qui caractérise le souvenir par opposition à la fiction, sont-ils permutés dans la conscience en un sentiment de déjà vu, au lieu de rester ce qu'ils sont originairement²?" So also Titchener: "Wundt's theory is open to the objection urged against his theory of space. The blending of affective process with sensation means, elsewhere in the mental life, not time but feeling; and we cannot understand how, in this particular case, the new product should arise³." Every criticism of the insufficiency

¹ E. R. Jaensch, "Ueber die Wahrnehmung des Baumes," *Ztsch. f. Psychol. Erg.-bd.* 6, 1911, 357.

² D. Katzaroff, "Contribution à l'étude de la Recognition," *Arch. de Psychol.* 1911, xi, 15, cf. also p. 19 and elsewhere.

³ E. B. Titchener, *Textbook of Psychology*, 1910, 347.

of mere association and the hopelessness of all attempts to come through with its aid alone are founded on this first principle of integration. Reid's answer to Hume's scepticism is the first step towards recovery from failure to do justice to the facts. The facts must be recognised. But this acceptance cannot now be framed so as to exclude further inquiry. For if some plausibility of derivation, some sort of resemblance, is what we desire, on finding it we necessarily accept the task of making an inductive study of these resemblances and of furnishing as adequate a theory of derivation as possible.

The second principle of integration is not by any means generally conceded. In fact it is usually implicitly denied. But whatever beliefs or prejudices may oppose it, it is the inevitable consequence of a systematization of the sensations and an essential part of any scientific psychology. It calls, of course, for the fullest experimental study of each mode of experience, both in respect of phenomenology and of function. The greater the disinterested devotion applied to its study, the more likely is it to be confirmed. For it promises the coincidence of broad rational demands with the facts, if only we treat the facts exhaustively enough. The psychology of the day presents many cases of difficulty and of opposition between reason and fact which call urgently for resolution.

The insight into the third principle is clouded by all sorts of philosophical generalities regarding continuity which do not attempt to define or to delimit precisely the mode of operation of the principle of continuity or to reconcile the demand for continuity with other legitimate demands. But the continuity and coherence are there. We do not need to create them; we have only to recognise them as they are, and to explain them. Recognising them for what they are cannot, however, mean attempting to maintain that experience brings no progress, no enrichment, nothing new, nothing more than was already within its compass. It is equally futile to barter the facts for a notion of self-development, or of the realisation of an end, as if that were a form of process in which all that is finally attained were already there from the lowliest form of consciousness, and so satisfied a craze for barren continuity. For purposive process in experience is itself undoubtedly a unique form of process, which therefore no more offers a standard for all other forms of integration than does any other unique process. If the continuity is there, we must just study it as we can and by inductive procedure extract from it what secrets it has to yield. Similarity is surely a kind of continuity. Whether it will suffice to cover the facts,

only detailed study can tell. But that it plays an important part in them, cannot be denied.

This third principle is indispensable in the formation of any theory that exceeds the bounds of sensationalism or its analogues. But it would be one-sided without the balance of the other two principles. Mere *nova* are inexplicable, whereas *nova* within a matrix of similarity offer the hope of an approximation towards completeness of theory. Even if distance were procured by a sort of sensory presentation of the orders intervening between those which bound it¹, it could not be thought, as distance, to be a mere aggregate of orders, for it is more than that. It integrates these orders in a special way, which can only progressively be exhausted by knowledge.

This principle has another important aspect. It offers a basis for the separation of the objective mind and its processes from the subjective mind of effort, assent, attention, and the like. If we know that we have the objective mind before us at any point, we can hope to determine its scope progressively by following out the various steps of its integrative development. There is evidence that the processes of integration can be influenced in various ways more or less extensively by the attention, but it must be just as erroneous to suggest that they originate in the processes of attention², as it would be to adopt the view that the mind involves only processes of integration of the kind found in the senses or in the cognitive states. If attention is involved in integration, it can only be supposed to support or to oppose the process of integration. It is not likely that the objective mind is a sort of image or parallel of the subjective mind of attention. Such a thing would not only be hardly intelligible, but it would refer or transfer all the problems of the objective mind to a shadowy world of subjective attention without any prospect of ultimate solution.

¹ Cf. Jaensch, *op. cit.* chap. 6.

² Cf. Jaensch, *op. cit.*, especially chap. 5.

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PUBLICATIONS RECENTLY RECEIVED

Mental and Social Measurements. By Professor EDWARD L. THORNDIKE. Second Edition—revised and enlarged. New York: Teachers' College, Columbia University. 1913. pp. xii + 277. \$2.50.

The second edition of this well-known work aims at presenting the student with a clearer, though perhaps more elementary, treatment of the subject than before; greater care being expended in describing the methods employed in solving statistical problems. As he writes in the preface to the former edition, "the author has had in mind the needs of the students of economics, sociology and education, possibly even more than those of students of psychology, pure and simple.... The book may, with certain limitations, be used as an introduction to the theory of measurement of all variable phenomena." It is intended for those who find the mathematical treatment, given in such books as Brown's or Yule's, too difficult.

Variations in the Grades of High School Pupils. By CLARENCE TRUMAN GRAY. Educational Psychology Monograph No. 8. Baltimore: Warwick and York. 1913. pp. 120. \$1.25.

By 'grades' are meant the percentage marks allotted by a teacher to his students in any subject. The special problems with which the writer here deals are (i) the variations in grading of the same pupils in different years and in different subjects of the high-school curriculum, (ii) the distribution-curves of the grades in different schools and in different subjects, (iii) the various methods of grading adopted by different teachers and by different schools, and (iv) the influence of home conditions on the variability of a pupil's grading from year to year. The chief value of the book lies in its attempt to provide "a relatively simple method by means of which any high-school principal can study the condition of the grading in his own school and take due steps to remedy the faults that he may find."

The Conservation of the Child: a manual of clinical psychology presenting the examination and treatment of backward children. By Dr ARTHUR HOLMES. Philadelphia and London: J. B. Lippincott Company. 1912. pp. 345. 4s. 6d. net.

The writer is assistant-director of the Psychological Clinic at the University of Philadelphia, which was established as long ago as 1896 by Professor Lightner Witmer, having for its objects the "collection and filing of data [obtained from mentally abnormal children]; the development of the best clinical tests for measuring the mentality of children; the training of teachers and social workers for service among mental defectives; the diagnosis of mental diseases [in the child]; and the most expeditious and satisfactory methods of connecting backward children with the proper sources of aid for relieving or ameliorating their condition." The book describes in detail the work of the clinic and the classification of cases brought there for investigation. But its frequent crudities make it better suited for the public, the general practitioner, and the school teacher, than for the trained psychologist, neurologist, and psychiatrist. A more serious work, written by a psychologist who has had fuller experience and received a medical education, would be of great value in this country. Here such a book as that of Dr Holmes is capable of doing considerable harm as well as good.

The Mental and Physical Life of School Children. By Dr PETER SANDIFORD.
London: Longmans, Green & Co. 1913. pp. xii + 346. 4s. 6d.

This book, says the author, "is intended to serve as a text-book, *i.e.* it aims at giving, in as brief a space as possible, a large number of facts which may be utilised in class discussion" by students in Training Colleges for Teachers. The first two sections are devoted to the physical life of the child and to the physiological basis of mental life. The next two sections are psychological, the one being headed 'dynamic or functional psychology,' the other 'descriptive psychology'; the author elects to include instincts and memory in the former, and emotions and perceptions in the latter section! The three remaining sections discuss the psychology of babyhood and adolescence, exceptional school children, and the development of language in children. Considering the wide ground covered, the author has compiled a useful and interesting little work. The defects are chiefly those inseparable from its 'scissors and paste' character.

Mental Fatigue. By Dr TSURU ARAI. New York City: Teachers' College, Columbia University. 1912. pp. 115. \$1.00.

In many cases "the subject of the experiment was the writer herself. But the danger that her presuppositions affected the results was precluded by the fact that the writer's knowledge of mental fatigue at that time was not enough to enable her to form any expectation of what form the fatigue curve in mental work would take." As might be expected, the results are very meagre and indefinite in comparison with the number and length of the experiments.

Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen. Von Prof. ERNST MEUMANN. Zweiter Band. Zweite umgearbeitete und vermehrte Auflage. Leipzig: W. Engelmann. 1913. S. xiv + 800. M. 11,- geb. M. 12.25.

The second of the three volumes which constitute the second edition mainly treats of the investigation of the individual mental differences in children, and the application and results of tests of mental and physical efficiency. No writer has yet attempted so complete a review of the now copious literature of these subjects.

The Interpretation of Dreams. By Prof. SIGMUND FREUD. Authorized translation of third edition with introduction by Dr A. A. BRILL.
London: George Allen & Co., Ltd. pp. xiii + 510.

A very readable translation of Freud's best-known work.

Psychoanalysis: its theories and practical application. By Dr A. A. BRILL.
Philadelphia: W. B. Saunders Co. 1913. pp. 337 13s. net.

This book presents a useful résumé of Freud's views, which are all uncritically accepted by the writer.

Memory: a contribution to experimental psychology. By HERMANN EBBINGHAUS. Translated by Prof. H. A. RUGER and C. E. BUSSENIUS. New York City: Teachers' College, Columbia University. 1913. pp. viii + 123. \$1.00.

This is a translation of the well-known pioneer work by the late Professor Ebbinghaus on the experimental psychology of memory, published in 1885. But any one with only a moderate knowledge of German will find the book more attractive and easier to read in the original than in its 'English' dress.

Prestige: a psychological study of social estimates. By LEWIS LEOPOLD. London: T. Fisher Unwin. 1913. pp. 352.

The interest of this book is ethnological and sociological; it contains nothing of psychological value.

The Psychology of Revolution. By GUSTAVE LE BON. Translated by B. MIALL. London: T. Fisher Unwin. 1913. pp. 336. 10s. 6d. net.

The main argument of the author, who deals chiefly with the French Revolution, is that all revolutionists have "obeyed invisible forces of which they were not the masters. Believing that they acted in the name of pure reason, they were really subject to mystic, affective, and collective influences, incomprehensible to them, and which we are only to-day beginning to understand." Nowhere does the book treat more intimately with the 'psychology' of revolution.

Modern Classical Philosophers: selections illustrating modern philosophy from Bruno to Spencer. Compiled by Dr BENJAMIN RAND. London: Constable & Co., Ltd. 1911. pp. xiv + 740. 10s. 6d. net.

This book "is virtually a history of modern philosophy based not upon the customary description of systems, but upon selections from original texts, and upon translations of the authors themselves."

The Classical Moralists: selections illustrating ethics from Socrates to Martineau. Compiled by Dr BENJAMIN RAND. London: Constable & Co., Ltd. 1910. pp. xx + 790. 10s. 6d. net.

"A companion volume in the field of ethics, to the author's 'Modern Classical Philosophers' in the domain of philosophy."

Die Praxis der Konstanzmethode. Von Prof. F. M. URBAN. Leipzig: W. Engelmann. 1912. S. 26. M. 1.

The writer here develops a *technique* of the method which he hopes, by the help of the tables he publishes and other means, will render it as generally used as the easier, though theoretically less satisfactory, methods of least perceptible differences and of mean error.

On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli. By Dr S. W. FERNBERGER. *Psychological Monograph*, Vol. XIV, No. 4. Princeton: Psychological Review Company. 1913. pp. 81.

The basis of this monograph is a series of experiments on two subjects, Dr Urban and the author, in lifting weights. Six variable weights were successively lifted, each with a standard weight of 100 grams, the standard weight being lifted first, and space errors being eliminated. The six comparisons furnished by this series provided data in the usual way for the constant method. But into this series was introduced a seventh weight, which, unlike the others, was changed at every lift in successive series in such a way as to afford data for the method of just perceptible differences by complete ascent and descent. By this procedure the two methods were worked simultaneously. The threshold values obtained by the two methods turn out to be extremely close, and, as might be expected, "the more nearly the experimental arrangement of the method of just perceptible differences approaches that of the method of constant stimuli, the closer do the values under discussion coincide." But this is regarded by the author as "a curious fact."

Zur Grundlegung der Tonpsychologie. Von Dr GÉZA RÉVÉSZ. Leipzig: Veit & Comp. 1913. S. viii + 148. M. 4; geb. M. 5.

The writer's experiments on the abnormal hearing of his friend Dr Paul v. Liebermann form the basis of these novel and important views on tone-psychology. He is led to distinguish the 'quality' of a tone sensation from its 'pitch'; all tones of the same name, e.g. c^1 , c^2 , c^3 , are qualitatively equal, but they differ widely in pitch. In the case of Dr v. Liebermann he believes that the pitch attribute was preserved although the quality attribute had become abnormal. To this subject a whole range of tones, from g^2 — $d^{\sharp 4}$ appeared of identical quality, viz. as g^{\sharp} . Which particular g^{\sharp} he heard depended on the 'pitch character' of the tone; thus tones between g^2 and b^2 were judged as $g^{\sharp 2}$, whereas tones between c^3 and b^3 were judged as $g^{\sharp 3}$. Thus the writer explains this subject's occasional answers that two successive tones in the affected region were 'not quite' a prime. As qualities they were identical, but their pitches were different. With the same subject the tone C_1 had a g quality. Consequently the notes C_0 , C_1 , given successively, were heard as a fourth. At the same time the subject admitted that the distance appeared much greater than a fourth—perhaps a major seventh or an octave. So too he declared that G_1 , C_1 , might be either a prime or an octave, explaining that the distance was too great for a prime and too small for an octave. Hence, says the author, what the subject heard was a g quality, preserving the pitch of C , but identical neither with G_1 nor with G_2 . In a case when d^3 gave the quality e , if c^3 and d^3 were sounded successively, the subject would at once judge the interval as a major third (c — e); if, however, the same tones were simultaneously sounded, the subject recognised by its degree of fusion that the interval was a major second. Thus the author separates the former judgment—which he calls one of interval and ascribes to differences in quality and pitch—from the latter to which he gives the name 'orthosymphony,' and which he ascribes to differences in pitch only. Differences in quality and their interrelation determine what he calls 'size of segment'; differences in pitch determine 'tone distances.'

Die Beziehungen der Psychologie zur Medizin und die Vorbildung der Mediziner. Von Dr W. PETERS. Würzburg: Curt Kabitsch. 1913. S. iv + 33. M. 1.20.

The author, a graduate in philosophy, follows several recent writers, e.g. Külpe and Marbe in Germany, and S. I. Franz, Adolf Meyer, Southard, Watson and Morton Prince in America, in urging that a prominent place should be found for psychology in the curriculum of the ordinary medical student. He traces the history of the development of psychology as an independent science, and treats of its connexion with physiology, pharmacology and psychiatry (with special reference to cerebral localization, Pawlow's conditioned reflexes, Korsakoff's psychosis, mnemasthenia, and hysteria), and its special interest for the school and prison doctor.

The Distinction between Mind and its Objects: the Adamson Lecture for 1913 with an Appendix. By BERNARD BOSANQUET. Manchester: University Press. pp. 73. 1s. net.

The author's main conclusions in this interesting lecture may be thus summarised. "Objects of finite mind, in short, and finite minds in themselves, are bound, after our discussion of physical realism, to strike us as details of reality essentially continuous with each other and reciprocally indispensable. But yet any object picked out and isolated within the whole is *eo ipso* not-mental, for you have taken it apart from the life of the whole, and have, by abstraction, killed and stuffed it for examination."

FREUD'S THEORY OF THE UNCONSCIOUS¹.

BY WILLIAM BROWN.

*From the Psychological Laboratory, King's College,
University of London.*

- I. *The general laws of mental process, as illustrated in dreams and hallucinations as well as in normal waking consciousness.*
- II. *Repression and wish-fulfilment.*
- III. *Psycho-analysis and hypnotism.*

FREUD summarises his fundamental views as to the nature and laws of working of the human mind, which he has formed on the basis of a detailed study of dreams and functional diseases, in the final chapter of the *Traumdeutung*. In fact, no one who has failed to master this most difficult chapter can justly claim any real insight into the theoretical and psychological aspects of Freud's work. It is only here that the exact meanings of such conceptions as 'wish-fulfilment' (*Wunscherfüllung*), 'repression' (*Verdrängung*), and the 'censor' (*Zensur*) are to be found, and the popular and figurative nature of much of the Freudian terminology is corrected. I shall therefore make this chapter the basis of my discussion.

I.

It is well known that dreams, like hysterical symptoms, are regarded by Freud as being the disguised fulfilments of repressed wishes². The manifest content of a dream is made up of a collection of memories from the waking life joined together by the most superficial forms of

¹ Read before Section I (Subsection of Psychology), British Association for the Advancement of Science, Birmingham, 1913.

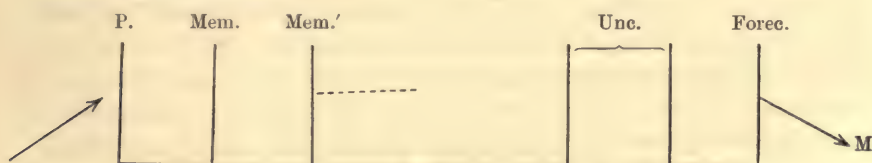
² For a detailed description of the dream-theory, see my two articles on "Freud's Theory of Dreams," *Lancet*, April 19th and April 26th, 1913.

association. Organic sensations and other sensory disturbances occurring during sleep, if not sufficiently intense to produce awakening, are either ignored or woven into the texture of the dream by arousing corresponding memories after the manner of an illusion. The method of psycho-analysis, to which we shall refer again later, enables us to find a meaning for this dream-formation in a set of latent dream thoughts which are invariably of the nature of wish-fulfilments. Since, according to Freud, the repressed wishes to which hysterical symptoms likewise point are always derived from an infantile source, he is strongly inclined to the view that the dream wishes are also either themselves infantile wishes or else wishes analogous to and sustained by wishes dating from the period of early childhood. He admits that this view has not yet been conclusively proved, but contends that it cannot be disproved. A large proportion of the dream-interpretations hitherto made do, as a fact, point to infantile wishes as the underlying motive power.

The discrepancy between the manifest dream content and the latent dream thoughts is due primarily to the resistance of the endopsychic censor. In order to evade this resistance and reach consciousness, the latent wishes undergo certain changes which may be summed up in the words 'condensation,' 'displacement,' 'dramatization,' and 'secondary elaboration.' In 'condensation' the numerous dream thoughts are replaced by a much smaller number of ideas selected because they act as nodal points in many intersecting trains of ideas and allude to these rather than directly represent them. 'Displacement' refers to the shifting of psychic accent from one part to another of the manifest content whereby the direct correspondence between it and the latent content is masked. Affects may also be displaced to produce the same result. 'Dramatization,' or regard for dramatic presentability (*Rucksicht auf Darstellbarkeit*), is provided for by the important process of 'regression,' in which the dream thoughts are reduced to their raw material, viz. sensory (chiefly visual) presentations, of hallucinatory vividness. These three changes constitute what is known as the 'dream-work,' and are characteristic of a form of mental activity neglected by normal psychology and almost unknown to normal waking consciousness, but identical with that responsible for the symptoms of hysterical patients. The fourth change, 'secondary elaboration,' is a process akin to that of waking consciousness, being an attempt to rationalise these strange and perplexing dream-formations and knit them up into a story or event with some degree of coherence. This

process continues after the dreamer awakes, and is one of the causes of the falsification of his memory of the dream during the following day.

Freud, in his attempt to form a general explanatory system within which these various psychical processes may be co-ordinated and rendered intelligible, finds it convenient to approach the subject with a number of 'auxiliary ideas' which, like auxiliary equations in mathematics, act as a sort of scaffolding in the discussion and are to be abandoned or drastically modified later on, according to the needs of the argument. Borrowing from Fechner the idea of a difference of 'psychic locality' in dream-formation, he conceives the mind as a system made up of a number of subsidiary systems placed in a definite order one behind another, so that mental activity will involve the excitation of these systems in a definite sequence. The spatial idea is here used metaphorically and has no necessary relation to the neural changes underlying mental activity. The following diagram¹ sums up this first tentative hypothesis:



P. represents the perceptual system, devoid of memory. Mem. is the system conserving the lasting traces of individual perceptions, in front of which are situated a series of other memory-systems, Mem.' corresponding to the various forms of association between these memories—simultaneity, similarity, etc.—and also, presumably, to higher thought-relations. Normal functioning of the psychical apparatus involves a transmission of excitation from the perceptual system in a progredient direction through the various memory systems to discharge itself eventually in motor innervation. But Freud draws an important distinction between two classes of memories or unconscious processes, one of which (Forec.) is in more immediate relation to movement (M.) than the other (Unc.). It is called the 'preconscious' (*das Vorbewusste*) or foreconscious, and the other is the 'unconscious' (*das Unbewusste*). Excitations in the preconscious can reach consciousness and pass over to movement so soon as they attain a certain degree of intensity and thus attract sufficient *attention* to themselves. Those in

¹ *Traumdeutung* (Brill's translation), 429.

the unconscious can only reach consciousness and control of the motor system by passing through the preconscious. In so doing they undergo certain changes. If, however, one inferred from this that, according to Freud, the preconscious is 'the censor' (*die Zensur*) of the dream-theory, one would probably be wrong, for he distinctly refers to the latter as "the resistance watching on the boundary between the unconscious and the preconscious¹," and in several other passages seems to make it clear that his conception of the censor is that of a 'non-conscious resistance' situated between the two systems of the unconscious. There is also a second censor between the preconscious and consciousness². For consciousness, in Freud's view, is to be regarded as simply a "sense organ for the perception of psychic qualities³," and even ideas in the preconscious may, if objectionable, be denied entrance to consciousness. The various processes we have been hitherto describing, and in fact all those of which the mind is capable, are to be regarded as running their course independently of consciousness. The true function of consciousness will be revealed later, when we come to closer grip with the central problem of psychological explanation. We may, however, conveniently quote at this stage of our discussion the significant words of Freud on 'unconscious psychical process': "Everything conscious has its preliminary step in the unconscious, whereas the unconscious may stop with this step and still claim full value as a psychic activity. Properly speaking, the unconscious is the real psychic; *its inner nature is just as unknown to us as the reality of the external world, and it is just as imperfectly reported to us through the data of consciousness as is the external world through the indications of our sensory organs*⁴." In this passage Freud is using the term 'unconscious' in the wider sense subscribed to by many modern psychologists, but in his own conception of the unconscious, as distinguished from the preconscious, we have an entirely new contribution to psychological theory. Freud's unconscious comprises the memories and mental processes of very early childhood, which have been repressed or abandoned in later life but which still retain their power of indirectly influencing consciousness by transferring the energy at their disposal to analogous ideas repressed from the preconscious, thus making these also unconscious.

Before leaving this first approximation to an explanation of the working of the mind, we may use it to illustrate what is meant by

¹ *Op. cit.* 430.

² *Op. cit.* 490.

³ *Op. cit.* 121, 453, 488.

⁴ *Op. cit.* 486. For a similar view of psychology as the science of the unconscious, see my "Epistemological Difficulties in Psychology," *Proc. Aristot. Soc.* 1909-10, x. 63-76.

'regression' in the Freudian system. Regression occurs when the excitation within the psychical apparatus takes a regressive instead of a progressive direction. This is, in Freud's view, the cause of the hallucinatory nature of dreams, and indeed of all hallucinations. Repelled by the censor and attracted by infantile wishes in the unconscious which transfer to them their energy, the latent dream thoughts abandon the progressive path through the preconscious towards movement and consciousness, and pass backwards through the various memory systems until they reach the perceptual system. The intensification necessary for this penetration to the perceptual system is mainly accounted for by the processes of condensation and displacement, although in the case of dreams the cessation of the progressive stream of excitation present in waking life is a contributory factor. In this way consciousness is aroused at the sensory end of the apparatus, and the dream has succeeded in evading the censor rather than surmounting it. The lowered activity of the censor during sleep, which Freud also assumes, only explains the formation of those few dreams which lack the dramatic character, and come to consciousness as thoughts, not as images. These pursue the progressive course throughout. In regression, on the other hand, "the structure of the dream thoughts is broken up into its raw material¹," and the thoughts are transformed into images.

As an instance of a hysterical hallucination produced by the same mechanism we may mention the case, given by Freud, of a twelve-year-old boy who was prevented from sleeping by a terrifying vision of green faces with red eyes. This hallucination corresponded to a suppressed memory, dating four years back, of a boy companion who had taught him many bad habits, including onanism. The patient's mother had remarked at the time that this boy had an unhealthy greenish countenance and red-rimmed eyes, and warned her little son that such wicked boys became backward at school and die young.

The explanations of hallucinations given in psychological text-books are for the most part physiological in nature and tend to slur over, if indeed they do not ignore, the problem of the 'meaning' of the hallucination. Thus James² explains these phenomena in the following way: The sensory vividness of an actual percept is due to (or, rather, correlated with) the passage of afferent nerve currents at high potential across the synapses of the sensory centre in the cerebral cortex. A

¹ *Op. cit.* 431.

² *Principles of Psychology*, 1890, II. 123, 124.

mental image, on the other hand, lacks sensory vividness because it is due to the excitation of the sensory centre by nerve currents of low potential flowing along association fibres from other parts of the cortex. While falling asleep, however, or under abnormal conditions in waking life, the synaptic resistances of the centre increase, so that the nerve-currents of low potential which are continually flowing to it along association paths can no longer pass through it and drain away into efferent fibres. The result is that nervous energy accumulates, the potential of the nerve-currents rises until it once more overcomes the synaptic resistance and produces an 'explosive discharge' of the nerve-cells corresponding in intensity to that accompanying perception. Hence the subjective hallucinatory experience. This theory assumes an identity of physiological site for the percept and the corresponding mental image, whereas for Freud the P system and the Mem. system are quite distinct, since he considers that the former must be quite devoid of memory if it is to perform its functions adequately. Moreover Freud does at least attempt to explain why certain mental contents are chosen to form an hallucination and not others. Those are selected which are recent and in themselves unimportant, since they have not had time or opportunity to enter into far-reaching associative connexions in the preconscious, and therefore are suitable material to receive the 'transference' (*Uebertragung*) of energy from desires in the unconscious. Their unimportance and superficial connexions with one another also protect them from the censorship.

The idea of regression is also to be found in McDougall's explanation of hallucination. McDougall writes: "It is known that in many cases of hallucination there is chronic irritation of a sense-organ; in cases of auditory hallucination, for example, it has sometimes been found that there is disease of the ear leading to continual irritation of the sensory neurones. We may suppose that disease induces an irritable weakness of a certain system of paths in one of the sensory areas of the cortex, so rendering them paths of abnormally low resistance, and that any impulses passing up from the corresponding sense-organ, and possibly also from other sense-organs, are therefore liable to be diverted to them from their normal paths, *so re-exciting the chains of cortical neurones in their whole length*, and producing a representation of sensory vividness¹." In Freud's theory, however, such an "irritable weakness of a certain system of paths" would not in itself suffice to produce the hallucination

¹ W. McDougall, *Physiological Psychology*, 86 (*italics mine*).

without the aid of energy from the powerful wishes of the unconscious which also determines the exact form which it shall take.

II.

In attempting a more accurate statement of his theory, Freud attributes to his unconscious and preconscious systems two different kinds of psychical process, viz. a 'primary process' and a 'secondary process' respectively. The one fundamental difference between these, which accounts for all the others, is that the secondary process is capable of 'inhibition' while the primary is not. The primary is the primitive and infantile, although even in earliest childhood it is probably not entirely unaccompanied by at least the germs of the secondary process. Its activity is limited to that of 'wishing,' and it strives to satisfy desire solely by reviving the memories of previous satisfactions and by intensifying them to hallucinatory vividness. Since permanent satisfaction is not to be obtained in this way, the mind has had to develop a secondary process which treats the memory of a previous satisfaction not as an end in itself but merely as a means to a more round about process of reinstating the actual satisfying object, or one like it. The primary process strives after a 'perception identity,' the secondary after a 'thought-identity.' In both cases the motive power is a wish, since, as Freud says, "nothing but a wish can impel our psychic apparatus to activity".¹

But Freud's fundamental explanatory principle is that of a *Besetzungsenergie*, or 'occupation energy,' which is subjected to different distributions within the psychic apparatus under different circumstances. Within the system of the unconscious this occupation energy is capable of a complete displacement from one presentation to another, so that ultimately one or a few presentations, which may be regarded as representing the rest, become sufficiently intense to penetrate to the perceptual system of the psychic apparatus. This is, of course, the primary process of wish-fulfilment; and the processes which we have hitherto classified under the heading of the 'dream-work' are nothing but aspects of the primary process. It is the same process which is responsible for the symptoms of hysteria, where the effects of condensation ('identification' or 'composition') and regression are clearly visible.

¹ *Op. cit.* 447. This sentence indicates one fundamental weakness of Freud's system, since *conations* below the ideational level are, of course, motive forces of the mind.

The distribution of 'occupation energy' under the influence of the secondary process is quite a different one. Freud writes:—"The manifold activity of the second system, tentatively sending forth and retracting energy, must on the one hand have full command over all memory material, but on the other hand it would be a superfluous expenditure for it to send to the individual mental paths large quantities of energy which would thus flow off to no purpose, diminishing the quantity available for the transformation of the outer world. In the interests of expediency I therefore postulate that the second system succeeds in maintaining the greater part of the occupation energy in a dormant state and in using but a small portion for the purposes of displacement¹." This is what he calls regulation by the 'principle of the smallest expenditure of innervation' (*Prinzip des kleinsten Innervationsaufwandes*).

Another principle which is obeyed by *both* systems is the 'principle of pain' (*Unlustprinzip*). This is simply the deviation of the psychic process from any memory involving pain. By virtue of it, "the first system is altogether incapable of introducing anything unpleasant into the mental associations. The system cannot do anything but wish²." Such a mere turning away from a painful memory is "the model and first example of *psychic repression* (*Verdrängung*)."

The second system retains control over painful memories in the face of this principle by so 'occupying' them that the pain attaching to them—which, like pleasure, is an efferent process analogous to a motor or secretory innervation—is almost completely inhibited. Now, owing to the insufficient development of the secondary process in the first two or three years of childhood, the memories and wishes of this period remain beyond control and inaccessible to the consciousness of later life. Some of these unconscious wishes are in conflict with the later wishes of the preconscious, so that their fulfilment would now produce pain instead of pleasure; "and it is just this transformation of effect" says Freud, "that constitutes the nature of what we designate as 'repression,' in which we recognise the infantile first step of passing adverse sentence or of rejecting through reason³." An example of this transformation of affect is the appearance of 'disgust' at a certain point in infantile development while previously absent.

These unconscious infantile memories are the precondition of all later repression. They are able to transfer their energy to any neglected

¹ *Op. cit.* 475.

² *Op. cit.* 476.

³ *Op. cit.* 479.

or suppressed thoughts of the preconscious whose content may happen to stand in some relation with their own. The preconscious then turns away from these thoughts of transference in accordance with the principle of pain and thus they are, as it were, drawn into the unconscious. This deviation from thoughts 'capitalised' by wishes in the unconscious is what is known as 'repression.' We thus see that 'repression' (*Verdrängung*) is not quite the same thing as 'suppression' (*Unterdrückung*), and has a definite technical meaning of its own in the Freudian system of psychology.

The repressed thoughts originating from the preconscious are now strong enough to persist in an independent and unconscious existence of their own, but can only attain to consciousness, if at all, by pursuing a regressive course and reaching the perceptual system. This is the way in which hysterical symptoms—paralyses, anaesthesias, aphonias, tics, contractures, convulsions, obsessions, phobias, etc.—originate, although it appears that another universal condition of their production is that a counter-wish from the preconscious, generally of the nature of a self-punishment, should also be present and fulfilled by the same symptoms. Hysterical symptoms are thus to be regarded as 'compromise-formations,' satisfying as well as may be a wish from the preconscious and one or more wishes from the unconscious. The dreams of normal persons are exactly analogous to such symptoms, being a compromise between the wish to sleep of the preconscious, and unconscious wishes aroused during the previous day or in the course of the night.

An essential part of Freud's theory of the psycho-neuroses is the view that "only sexual wish-feelings from the infantile life experience repression (emotional transformation) during the developmental period of childhood¹." These are directed towards the parents, or their substitutes, and constitute the well-known 'Oedipus complex' or 'Electra complex,' according to the sex. It is because they are capable of an organic re-inforcement in later life, especially at the time of puberty, that they endanger the mental equilibrium as no other tendencies can do. Space does not permit me to make more than this very inadequate reference to Freud's sexual theory in the present paper, although its importance for a true appreciation of his entire system can hardly be overestimated. As regards the dreams of normal persons, Freud prefers to leave it undecided whether these are ultimately based upon sexual wishes of the unconscious². Indeed in some passages of the *Traumdeutung*, he definitely

¹ *Op. cit.* 480.

² *Cf.* p. 481.

leans towards the view that tendencies like hunger, thirst, and the desire for power are fully competent to produce dreams without further aid from the unconscious. Anxiety dreams are certainly sexual in significance, if not always so in origin. The feeling of anxiety is due to an overpowering of the second system by the first, and indicates a failure in that 'compromise' to which we have already referred. Thus the function of compromise-formations, such as dreams and hysterical symptoms, is to guard against the outbreak of anxiety. Freud illustrates this by reference to the case of agoraphobia. "Suppose a neurotic incapable of crossing the street alone, which we would justly call a 'symptom.' We attempt to remove this symptom by urging him to the action which he deems himself incapable of. The result will be an attack of anxiety, just as an attack of anxiety in the street has often been the cause of establishing an agoraphobia. We thus learn that the symptom has been constituted in order to guard against the outbreak of the anxiety. The phobia is thrown before the anxiety like a fortress on the frontier¹." But in some cases the originating cause is the intense pain of certain organic sensations aroused during sleep, especially with people who suffer from disease of the heart or lungs. The anxiety thus somatically aroused gains a psychical interpretation in the dream by liberating unconscious wishes, the fulfilment of which in face of the censorship would be accompanied by a similar feeling of anguish.

III.

With regard to the use of 'symbols' in dreams, it is only necessary for us, in the interests of theory, to point out that these are not products of dream activity. The symbolizing tendency is already present in the latent dream thoughts, and is identical with that responsible for our myths and legends. The predominant use which the dream makes of such symbols is due to their dramatic fitness and their freedom from the censorship. Although certain of these symbols tend to have the same meaning among a whole class of individuals, it must never be forgotten that their significance in any single case can only be accurately determined by means of psycho-analysis. It is because Pierre Janet has failed to realise this that so much of his recent criticism of the Freudian school is unconvincing².

¹ *Op. cit.* 459.

² P. Janet, "Psycho-analysis," *XVIIth Internat. Congr. of Med.*, London, 1913, Section XII. 13-64. See especially p. 26 for the point here raised.

Psycho-analysis is something more than a mere catechizing of the patient. Experience has shown that certain memories which are inaccessible under ordinary circumstances will rise to the surface of the mind if the patient adopts an attitude of uncritical meditation and follows the sequence of associated ideas as they appear, rejecting none of them however painful, objectionable, or absurd they may seem to be. In the case of dream-interpretation, the separate sections of the manifest content are taken as the independent starting-points for these chains of 'free' associations; in the case of a psycho-neurosis the symptoms serve the same purpose. It is important to realise that these chains of ideas are not truly free or aimless associations. When, by adopting the attitude of uncritical reverie, the patient succeeds in freeing himself from 'consciously purposeful mental activity' (*bekannte Zielvorstellungen*), his mind does not cease to be purposive but is now dominated by 'unconscious trends of activity' (*unbewusste Zielvorstellungen*) which determine what ideas shall rise to consciousness. The ideas which in this way are eventually reached are found to allude to, if not to form an integral part of, a system of preconscious thoughts which had by transference been dragged into the unconscious and which constitute the interpretation of the dream or the psycho-neurotic symptom, as the case may be. The process of psycho-analysis, by bringing these thoughts once more under the control of the preconscious, *ipso facto* brings about the resolution of the hysterical symptoms and the cure of the patient. It is in this sense that we are to take the dictum of Breuer and Freud that "solution and treatment go hand in hand". The course of treatment is as a rule a lengthy one and makes considerable demands upon the tact and energy of the physician, since the trains of associations are being continually interrupted by 'resistances' which the patient is unable to cope with single-handed, despite his best intentions, and it is only with the aid of persistent urging on the part of the physician that the hindrances are overcome and the ideas again continue to flow. In order that the cure may be complete the patient must be able to live again through the intense emotions attached to the repressed ideas and direct them upon the personality of the physician. This indispensable cathartic process is known as 'abreaction' (*Abreagierung*).

The well-known 'word-association method' of C. G. Jung is very useful as an adjunct of psycho-analytic procedure, and in the case of some of the psychoses is the only suitable method. It serves to indicate

¹ *Op. cit.* 83.

the principal unconscious 'complexes,' i.e. systems of repressed and emotionally-tinged ideas, from which the patient is suffering. The clearest and most frequent sign of the existence of such a complex is: (1) *a prolonged reaction time*, but it should not be forgotten that there are other 'complex-indicators' of equal importance. These are: (2) *a failure to react*; (3) *an over-reaction*, giving more than is asked for, many words, with supplementary explanations, instead of one; (4) *a repetition of the stimulus word*; (5) *an identical word-reaction to the most varied stimulus words*; (6) *a superficial association*, especially if combined with a prolonged association time; (7) *a meaningless reaction*; (8) *an assimilation of the stimulus word*, where it is misread, misunderstood, or taken in an unusual sense under the influence of the complex, being thus 'assimilated' to the complex; (9) *a failure in reproduction*, the patient giving a different reaction-word on a second presentation of the stimulus-word, although asked to reply if possible with the same word as before¹. The chief theoretical interest of Jung's work on association is that he has succeeded in giving an experimental proof of the validity of the main assumptions upon which Freud's psycho-analytic technique is based.

The relation of psycho-analysis to hypnotism is a problem of great interest, which I am inclined to think is still awaiting solution, despite the claim to a satisfactory understanding of it made by the Freudians. Ferenczi² has carried out psycho-analyses of patients whom he had previously treated by hypnotism, and considers that the results confirm Freud's view that in hypnotism unconscious sexual tendencies of the patient are 'transferred' from their original object, the parent, to the person of the hypnotist. "Hypnosis is a special form of artificially increased *suggestibility*," and suggestibility is nothing more than the survival in the unconscious of the child's readiness to believe blindly and obey uncritically those whom it loves. Now, since the symptoms in hysteria are likewise perverted satisfactions of psycho-sexual wishes emanating from the infantile unconscious, it follows that the removal of such symptoms by hypnosis or by the milder forms of suggestion is merely a case of replacing them by another symptom, viz. "psycho-sexual dependence upon the physician." For this reason hypnotic cures

¹ Some results obtained by this method will be found in a short paper by me, "A Case of Extensive Amnesia of Remote Date cured by Psycho-Analysis and Hypnosis," *Brit. Med. J.*, Nov. 8th, 1913.

² Ferenczi, "Introjektion und Uebertragung," *Jhrb. f. psychoanal. u. psychopath. Forsch.* 1909, i. See also Ernest Jones, "The Action of Suggestion in Psychotherapy," *J. of Abnorm. Psychol.* Dec. 1910, v.

are so seldom permanent. Psycho-analysis, on the other hand, avoids this unsatisfactory result by dragging up the psycho-sexual tendencies into consciousness, so enabling the patient to understand their true nature and to 'sublimate' them, *i.e.* direct them to useful social activities.

Janet considers that the attachment of the patient to his physician, upon which this theory is based, is not to be so simply explained. He writes: "Cet attachement se présente de bien des manières différentes et semble dépendre de phénomènes psychologiques très divers dans lesquels interviennent suivant les cas des suggestions, des aboulies, l'incapacité à conclure par soi-même, le besoin d'être compris, le besoin d'être dirigé et surtout le besoin d'être excité si important chez les déprimés¹." Only on the assumption that every form of docility is sexual in origin can Ferenczi's theory lay claim to truth. The question is largely one of fact, and although Janet's extended and world-famed experience as a hypnotist lends great weight to his words, we cannot overlook the empirical results of psycho-analysis; and if Ferenczi's comparative investigations are confirmed by independent and *unbiassed* observers, his theory must be accepted. Even then a number of outlying questions of great importance remain to be answered. For example, what is the cause of the remarkable broadening of the field of consciousness and improvement of memory that occur in the hypnotic state, prior to any suggestions made by the hypnotist? In the case referred to on page 276 of an extensive amnesia of thirteen years standing—the loss of memory covered the period from September, 1897, to February, 1900—almost all the essential memories reappeared directly the first hypnotic slumber had been induced, without any special prompting from myself. I had, during the previous fortnight, plied the patient repeatedly with word-association tests without much apparent success, but am inclined to think that this treatment acted as a very powerful predisposing influence towards hypnosis, since the patient, who had never been hypnotized before and had repeatedly expressed great scepticism as to anyone, myself included, being able to hypnotize him, went into the hypnotic trance with complete loss of consciousness in less than ten minutes. Moreover, in the course of the word-association tests he frequently forgot the stimulus-word, and sometimes also the reaction-word, immediately after replying. This suggests a close relation between the state of hypnosis (before any suggestions

¹ P. Janet, *op. cit.* 38.

have been given) and the state of mind during psycho-analysis,—a relation which has not escaped Freud's notice, for he writes (of psycho-analysis): "As may be seen, the point is to bring about a psychic state to some extent analogous as regards the apportionment of psychic energy (transferable attention) to the state prior to falling asleep (*and indeed also to the hypnotic state*)¹." This resemblance is worthy of further investigation.

The remarkable physiological manifestations often observed in hypnotized subjects also still await an explanation that will be completely satisfactory to the scientific mind. The Freudians may retort that these are identical in nature with the symptoms of conversion-hysteria, thus agreeing with the dictum of Charcot that "hypnosis is an artificial hysteria." But this does not help us much, for the wish-fulfilment theory merely indicates the psychical significance of these symptoms; the psycho-physiological or purely physiological changes which occur in the nervous system must form an integral part of any complete causal explanation. Freud is fully alive to this lacuna in his theory, though his disciples tend to push it into the background and often ignore it completely. He writes in reference to the inhibitory functions of the secondary process: "The mechanism of these processes is entirely unknown to me; anyone who wishes to follow up these ideas must try to find physical analogies and prepare the way for the visualising of the dynamic process (*Veranschaulichung des Bewegungsvorganges*) in (the theory of) the stimulation of the neuron. I merely hold to the idea that the activity of the first psychical system is directed *to the free outflow of the quantities of excitement*, and that the second system brings about an inhibition of this outflow through the energies (*Besetzungen*) emanating from it, *i.e.* it produces a *transformation into dormant energy (ruhende Besetzung) involving a raising of the level*²." He nowhere says whether his *Besetzungsenergie* is mental or physical, but we can hardly refuse to assume that it has at least a physiological correlate in the form of nerve-energy; and since he definitely states that the systems of the psychic apparatus have nothing psychic in themselves³ being analogous to the lenses of a telescope which produce virtual images corresponding to the objects of internal perception (*i.e.* psychical objects), it is only to the anatomy and

¹ *Traumdeutung* (Brill's translation, 85; italics mine).

² *Op. cit.* 475. I have made two slight alterations in Brill's translation.

³ See p. 484.

physiology of the central nervous system that we can turn for further explanation.

Now it seems to me that McDougall's interesting theories as to the physiological processes underlying psychical activity throw much additional light on the psycho-physics of inhibition, repression and symptom-formation¹. McDougall regards the passage of nervous energy (*neurokyme*) across the synapses of the cerebral cortex as the physiological correlate of psychical process, and would explain inhibition as a secondary effect of the act of attending. In attending to one object or concentrating the mind on one form of self-activity, neurokyme is concentrated, raised to a higher potential, in a particular system of neurons, and by virtue of the lowered resistance of the intervening synapses *drains* energy from all neighbouring systems along collaterals which extend from their neurons to these synapses. McDougall supports this theory by numerous observations on the psychology of sensation and perception that are not easily explained in any other way. He regards the special inhibitory nerves connected with the autonomic nervous system as a primitive device which has been superseded by this more efficient mechanism in the course of evolution of the central nervous system. The repression of a mental tendency would thus correspond to a withdrawal of neurokyme from the correlated system of nerve-arcs; and the resistance of the censor would correspond to an actual heightened resistance of synapses that divide the wide system of interrelated sub-systems functioning as the preconscious from that functioning as the unconscious. It is at least probable that Freud means by the censor something unconscious, for in his analogy of the telescope he compares it to the "refraction of rays in their passage into a new medium²."

McDougall's theory will probably need much further elaboration and (possibly) modification to make it fit all the facts now known about functional diseases. That the altered conductivity of certain synapses plays a decisive part in the causation of these disorders there can be little doubt. I recently had the opportunity of observing a case of hysterical astasia abasia³ in a woman patient over forty years old, the immediate or occasioning cause of which was an operation for

¹ W. McDougall, "The Seat of the Psycho-Physical Processes," *Brain*, 1901, xxiv.; "The Nature of Inhibitory Processes within the Nervous System," *ibid.* 1903, xxvi.; "The State of the Brain during Hypnosis," *ibid.* 1908, xxxi.

² *Op. cit.* 484.

³ A functional inability to stand or walk.

appendicitis. The woman had to re-learn, slowly and painfully, the art of walking which she seemed to have completely forgotten. But close observation showed that the chief feature of the symptom was a lack of co-ordinating power of a particular kind. Whereas in normal walking contraction of the flexor muscles is accompanied by automatic relaxation of the corresponding extensors, and *vice versa*, in accordance with Sherrington's law of reciprocal innervation, here contraction of both sets occurred simultaneously. The patient while putting her leg forward seemed at the same time to be trying to draw it back, and similarly with other movements.

Since normal reciprocal innervation is best explained by McDougall's theory as a reciprocal inhibition (this being caused by the drainage of innervation energy from the less intensely charged chain of neurons to the neuron-chain carrying the increased innervation necessary for the initiation of a movement), our case is one of functional disturbance of this mechanism in the form of altered resistances at the synapses. Paralyses, contractures, and in fact all motor symptoms observable in hysterical patients may be physiologically explained in exactly the same way. It is a short step from this to a similar explanation of sensory symptoms. Such explanation of course merely supplements, it does not exclude, a psychological interpretation in terms of 'meaning,' such as Freud gives.

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THE ANALYSIS OF SOME PERSONAL DREAMS, WITH REFERENCE TO FREUD'S THEORY OF DREAM INTERPRETATION¹.

By T. H. PEAR.

- I. *Introduction.*
- II. *Points in the dreams which bear a relation to Freud's theory.*
- III. *Some remarks on Freud's theory.*
- IV. *The dreams ; their analysis and interpretation.*
- V. *Conclusions.*

I. INTRODUCTION.

THE last few years have seen a noteworthy change in the attitude of psychologists towards the dream. It may fairly be said that from any existing text-book of general psychology one can gain very little knowledge on this subject, which has always been of intense interest to the non-scientific public. Even the sparse details which may be gleaned are usually of such a vague and general nature that they are of little use in helping the psychologist to understand the relation of the dream to other mental processes, particularly to those of normal and abnormal waking life. Since the publication of Freud's theory², however, the study of dreams has naturally received a great impetus. But it is scarcely necessary to point out that the examination of a theory so complex as this will involve the investigation of a very large number of dreams of different people, in order to ascertain the extent to which the dreams of various persons exhibit individual differences, as well as the nature of these differences, and their relation to different types of mind.

The following article will attempt to analyse in detail, and to account for, two dreams of the writer, and it will indicate their relation to

¹ Amplified from a paper read at the meeting of the British Association for the Advancement of Science, Birmingham, September, 1913.

² The *Interpretation of Dreams* (Translation by Brill of the 3rd edition of *Die Traumdeutung*), London, 1913.

Freud's theory of dreams without entering into a detailed discussion of his *general* psychological theory, which is at present so much in debate. At the present stage of the controversy it seems more profitable to examine minutely fact after fact of mental life by which the special validity of the single parts of his theory may be tested, and to postpone the examination of his theories as a whole until more evidence is forthcoming from the experiences of many normal persons, of widely different mental characteristics. That this evidence is by no means complete is apparent to anyone who is conversant with modern psychological and psycho-pathological literature¹, and this fact forms the justification for the appearance of this paper.

One of the chief objections frequently raised against Freud's theory of dreams is that the dreams upon which his explanations are founded were either his own or those of persons whose mental condition was so abnormal that at the time they experienced the dreams they were undergoing medical treatment on this account. It has sometimes been said that, even if the theory be true for abnormal patients it need not be valid as an explanation of the dreams of normal people. But, since it is generally admitted that mental normality and abnormality are separated only by an infinite number of gradations, it is impossible to believe that at some point in the transition the 'normal' set of laws gives place to 'abnormal' laws. Freud says², "The objection that no deduction can be drawn regarding the dreams of healthy persons from my own dreams and from those of neurotic patients may be rejected without comment." But, whatever may be the theoretical justification for this remark, it is obvious that we need a scientific statement of the dream-phenomena occurring in ordinarily healthy minds.

Up to the present time, only a few workers have paid careful and systematic attention to their own dreams. Their results have been of great value in many ways: they have studied the material of the dream, its images, thoughts and feelings³, but few of their findings can be used as a means of testing Freud's theory. In the first place they have, to use

¹ See M. Isserlin, "Die Psychoanalytische Methode Freuds," *Ztsch. f. d. ges. Neurol. u. Psychiat.* Bd. 1. Heft 1., also *Ergeb. d. Neurol. u. Psychiat.* 1911. A. Kronfeld, "Über die psychologischen Theorien Freuds und verwandte Anschauungen," *Arch. f. d. ges. Psychol.* xxii, 2, 3.

² *Op. cit.* 482.

³ Especially interesting examples of this kind of investigation have been recently furnished by F. Hacker, "Systematische Traumbeobachtungen mit besonderer Berücksichtigung der Gedanken," *Arch. f. d. ges. Psychol.* 1911, xxi, 1-3, 1-131, and P. Köhler, *ibid.* 1912, xxiii, 415-489.

a figurative expression, studied the minute anatomy of the dream rather than its behaviour; and few of them have paid attention to the dream when taken as a whole. This, however, is a point which is insisted upon by Freud. Secondly, most of these detailed studies are concerned with the 'apparent dream' (the 'manifest content' of Freud), and criticism of the dream theory on the basis of such work misses Freud's main point, viz. that his theory refers specifically to the 'latent content,' viz. the thoughts which are at the basis of the dream¹. So we still need a careful examination of the dreams of normal persons, noted without delay on awaking.

The two dreams recorded here occurred in the sleep just before awaking at the usual time in the morning, and in connexion with them it should be remembered that, as Hacker has pointed out², one cannot assume that the dreams of *deep* sleep are of this nature.

II. POINTS IN THE DREAMS WHICH BEAR A RELATION TO FREUD'S THEORY.

Full accounts of Freud's theory of dreams will be found in his own book and in articles on the subject by Ernest Jones³, Ferenczi³, and William Brown⁴. The main points of the theory which may be examined in the dreams analysed in this paper are the following:

1. The relation of the 'manifest content' to the 'latent content' or the dream thoughts.
2. The 'censorship' of consciousness.
3. The 'dream-work,' which produces the distortion necessary to evade the 'censor,' including the processes of dramatization, symbolism, condensation and displacement.
4. The dream as the fulfilment of a wish.
5. The dream as the disguised fulfilment of a repressed wish.
6. The relation in the dream of the conscious to the unconscious wishes.
7. The rôle of the infantile wish in the dream.

¹ Freud himself says in *The Interpretation of Dreams* (p. 114), "It is quite incredible with what stubbornness readers and critics exclude this consideration, and leave unheeded the fundamental differentiation between the manifest and the latent dream content."

² *Op. cit.* 123.

³ *Amer. J. of Psychol.* 1910.

⁴ *Lancet*, April 19th and 26th, 1913; also this *Journal*, 1914, vi. 265-280.

III. SOME REMARKS ON FREUD'S THEORY.

It is, unfortunately, not an easy matter to obtain a clear and unequivocal statement of Freud's own theory. The difficulty is increased when one consults the expositions of the theory by other workers. In the first place, Freud's own treatment of the subject is not free from inconsistencies. Questions on which, in the earlier part of his book, he expresses a guarded opinion are answered more dogmatically in the later chapters. In fact, the last chapter is rather a statement of opinion than a scientific treatment of the subject. This appears when we consider two important points:

(1) That the interpretations of his own dreams (which may be considered the most valuable evidence for his theory) in the earlier part of the book do not themselves form a factual basis for the extensions of the theory made in the later theoretical treatment.

(2) That the later chapters occasionally conflict with statements made in the earlier chapters.

It is instructive to take Freud's own statements concerning two most important points in his theory, viz. the relation of the unconscious to the conscious wishes in the dream, and the rôle of the infantile wish in the dream¹.

(1) *The Wish in the Dream.*

Page 100. "The dream represents a certain condition of affairs as I should wish it to be; the content of the dream is thus the fulfilment of a wish; its motive is a wish."

Page 102 (referring to the dream which was used on page 100). "I do not wish to claim that I have revealed the meaning of the dream entirely, or that the interpretation is flawless."... "When the work of interpretation has been completed the dream may be recognised as the fulfilment of a wish."

Page 107. "The dreams of little children are simple fulfilments of wishes..."

Page 436. "The undisguised wish-fulfilments were chiefly found in children, yet fleeting open-hearted wish dreams *seemed* (I purposely emphasise this word) to occur also in adults."

Page 438. "I have a strong doubt whether an unfulfilled wish from the day would suffice to create a dream in the adult. It would rather seem that as we learn to control our impulses by intellectual activity, we more and more reject as vain the

¹ I am well aware of the danger of unfair treatment in taking sentences out of their context, but I believe that in the cases cited this can be done with scrupulous justice, since the meaning of the sentences, and that of the connexions in which they occur, is so clear that misunderstanding seems impossible. The quotations are taken from the latest available edition of Freud's work cited on p. 281.

formation or retention of such intense wishes as are natural to childhood. In this, indeed, there may be individual variations ; some retain the infantile type of psychic processes longer than others. The differences are here the same as those found in the gradual decline of the originally distinct visual imagination.

In general, however, I am of the opinion that unfulfilled wishes of the day are insufficient to produce a dream in adults.—I believe that *the conscious wish is a dream inciter only if it succeeds in arousing a similar unconscious wish which reinforces it*¹."

(2) *The Infantile Wish in the Dream.*

If we examine the chief statements concerning the part played by the infantile wish in the dream, we find :

Page 160. "In another series of dreams we learn from analysis that the wish itself, which has given rise to the dream, and whose fulfilment the dream turns out to be, has originated in childhood,—until one is astonished to find that the child with all its impulses lives on in the dream."

Page 162. "Another case establishes the fact that although the wish which actuates the dream is a present one, it nevertheless draws great intensification from childhood memories."

Page 166. "The deeper one goes in the analysis of dreams, the more often one is put on the track of childish experiences which play the part of dream sources in the latent dream content."..."As a rule, of course, a childhood scene is represented in the manifest dream content only by an allusion, and must be extricated from the dream by means of interpretation. The citation of examples of this kind cannot have a very convincing effect, because every guarantee that they are experiences of childhood is lacking ; if they belong to an earlier time of life, they are no longer recognised by our memory. Justification for the conclusion that such childish experiences generally exist in dreams is based upon a great number of factors which become apparent in psychoanalytical work, and which seem reliable enough when regarded as a whole. But when, for the purposes of dream interpretation, such references of dreams to childish experiences are torn from their context, they will perhaps not make much impression, especially since I never give all the material upon which the interpretation depends."

Page 171. "My collection, of course, contains an abundant supply of such patients' dreams, whose analysis leads to childish impressions that are remembered obscurely or not at all, and that often date back to the first years of life. But it is a mistake to draw conclusions from them which are to apply to the dream in general ; we are in every case dealing with neurotic, particularly with hysterical persons, and the part played by childhood scenes in these dreams might be conditioned by the nature of the neurosis, and not by that of the dream. However, I am struck quite as often in the course of interpreting my own dreams, which I do not do on account of obvious symptoms of disease, by the fact that I unsuspectingly come upon a scene of childhood in the latent dream content, and that a whole series of dreams suddenly falls into line with conclusions drawn from childish experiences."

¹ The italics are those of Freud.

Pages 183-4. "Since I have learnt, further, from experience in dream analysis that there always remain important trains of thought proceeding from dreams whose interpretation at first seemed complete (because the sources of the dream and the actuation of the wish are easily demonstrable), trains of thought reaching back into earliest childhood, I have been forced to ask myself whether this feature does not constitute an essential condition of dreaming. If I were to generalise this thesis, a connexion with what has been recently experienced would form a part of the manifest content of every dream, and a connexion with what has been most remotely experienced, of its latent content; and I can actually show in the analysis of hysteria that in a true sense these remote experiences have remained recent up to the present time. But this conjecture seems still very difficult to prove; I shall probably have to return to the part played by the earliest childhood experiences, in another direction (Chapter VII)....The dream often appears ambiguous; not only may several wish-fulfilments, as the examples show, be united in it, but one meaning or one wish-fulfilment may also conceal another, until at the bottom one comes upon the fulfilment of a wish from the earliest period of childhood; and here, too, it may be questioned whether 'often' in this sentence may not more correctly be replaced by 'regularly.'"

Page 439. "I say that these wishes found in the repression are themselves of an infantile origin, as we have learned from the psychological investigation of the neuroses. I should like, therefore, to withdraw the opinion previously expressed that it is unimportant whence the dream-wish originates, and replace it by another, as follows: *The wish manifested in the dream must be an infantile one*¹. In the adult it originates in the Unconscious, while in the child where no separation and censor as yet exist between Foreconscious and Unconscious, or where these are only in the process of formation, it is an unfulfilled and unrepressed wish from the waking state, I am aware that this conception cannot be generally demonstrated, but I maintain nevertheless that it can be frequently demonstrated, even where it was not suspected, and that it cannot be generally refuted.

The wish-feelings which remain from the conscious waking state, are, therefore, relegated to the background in the dream formation. In the dream content I shall attribute to them only the part ascribed to the material of actual sensations during sleep (see p. 185)."

Page 447. "*The dream is a fragment of the abandoned psychic life of the child*¹."

Page 481. "*I will leave it undecided whether the postulate of the sexual and infantile may also be asserted for the theory of the dream; I leave this here unfinished because I have already passed a step beyond the demonstrable in assuming that the dream-wish invariably originates from the unconscious*²."

The last statement quoted shows, therefore, that it is strictly fair to conclude that in the development, in his book, of the two highly important assertions, viz. that

(1) (page 438), "I believe that the conscious wish is a dream-inciter only if it succeeds in arousing a similar unconscious wish which reinforces it," and

¹ The italics are those of Freud.

² The italics are mine.

(2) (page 439), "The wish manifested in the dream must be an infantile one,"

Freud has not proved his points. We see, too¹, that he is conscious of this omission.

If we examine some of the most striking examples of his own dreams we find that they are 'grown-up' dreams which are actuated by professional interests (cf. the second dream examined in this article), and in them he demonstrates no infantile factors, nor does he show that the wish at the bottom of these dreams was invariably a repressed, unconscious one. It is necessary to point out this fact, even at the risk of becoming wearisome, since later expositions of his theory by others state in a dogmatic manner what Freud himself expresses with diffidence. Jones², for instance, says "The latent content is always unconscious, *i.e.* it consists of mental processes unknown to the person, and of which he cannot become aware by direct introspection, but only by means of certain indirect modes of approach....The latent content is of infantile origin, later additions being merely reinforcements of earlier infantile trends."

It may be argued that subsequent work has justified this removal of the limitations originally proposed by Freud in the statement of his theory, but, so far as can be gathered from current literature, this work has been performed mainly upon psycho-neurotic patients, and Freud's own warning, with regard to this work when used as a basis for general assertions concerning the rôle of the infantile in the dream of the *normal* person, has already been quoted³.

It is quite clear then, that we need to know more about the dreams of normal persons before the question of the importance of the infantile unconscious wish in a general theory of dreams can be satisfactorily answered.

IV. THE DREAMS, THEIR ANALYSIS AND INTERPRETATION.

The first dream recorded below was noted immediately on awaking, and as at the time no writing material was available the incidents in it were repeated in words several times to himself by the writer until he knew it by heart. This dream was unusually vivid and easily remembered on awaking, and the immediate repetition, several times over, of the very few points in the dream, combined with the fact that as soon as possible it was recorded in writing, obviate the possibility of addition

¹ *Op. cit.* 481.

² *Papers on Psycho-analysis*, 1913, 367.

³ See quotation from Freud (page 171) given on page 285 of this paper.

to it. The second dream was recorded in writing immediately on awaking.

The analysis was carried out in the well-known way, by tracing the dream material to its sources in waking life through the serial association method, when the mind was freed from all criticism or conscious guidance of the ideas which came to consciousness. Both dreams have been submitted to psycho-analysis by a second person trained in psychology, but no dream thoughts other than those discovered by the method of 'free association' applied by the writer to himself¹, were found.

In the first dream I have, for obvious reasons, omitted the names, and altered the initials of the names, of the persons who appear in it and in the dream thoughts. It is with reluctance that I publish this dream, but the reason which impels me to do so is that I think it important because it was the first dream analysed by me, at a time when I knew only the bare outlines of Freud's theory. Further, at the time of noting it, every detail in it appeared to me to be perfectly remembered. When it was analysed every point seemed to be perfectly accounted for, in terms of my past experience. This is a subjective feeling which rarely occurs to me when considering my own dreams², but it was very clear at the time.

In the second dream, which is constructed around psychological matters connected with Dr C. S. Myers, I am permitted by his kindness to use his name without alteration.

The First Dream.

The Apparent Dream. I was in an attic with a rafted roof. On one side of the room the roof came down nearly to the level of the floor; on the other, it rose to a fair height. Psychological apparatus was dotted about the room. I was experimenting with some apparatus (the character of which I do not remember), being assisted by Miss G., a colleague at the University of Manchester. Miss G. said suddenly "It's one o'clock, let us go over to lunch³," and, moving over to some pegs, she took down a 'blazer' and put it on instead of her coat. The 'blazer' was maroon in colour, with two shields on the pocket. I looked surprised at this action, whereupon she said, "Oh, it doesn't matter what one wears over there." I awoke, laughing.

¹ See Freud, *op. cit.* 414.

² I have been studying my own dreams for the last 1½ years.

³ In the dream I understand this to mean "in the refectory."

After waking, the dream was at once considered, in the manner mentioned on pages 287, 288, and the 'free associations' to the various points of the dream were noted.

Sources of the apparent dream. (The items following in italics refer to the apparent dream.)

Attic, raftered roof. The size and shape of the room, and the slope of the roof, are those of my bedroom in Manchester, which is an *attic*. Shortly before the dream, I had been discussing a proposed extension of my laboratory, and a member of the staff had said to me, "One way would be to put you in the *attics* in the main building. Go and see if they will suit you." While this question of extension was still unsettled, *Miss G.* sent for me to discuss the possibility of my taking one of her classes in my *laboratory*. This change would necessitate the enlargement of my laboratory, and would make us members of the same department (*i.e.* the department of Education). We should thus *work together*. I was anxious, for several reasons, to teach this new class. While visiting her room, which is one of the *attics* under consideration, I had examined critically this room and the neighbouring ones. They have *raftered roofs*.

Coat, Miss G. A friend, S. (whose importance will appear later), had recently said to me, in talking of *Miss G.*, "She looked cold to-day at lunch, and was wearing her *coat* in the *refectory*." (The fact that the dream occurred in the summer should be mentioned here, as it accounts for the interest taken in this otherwise commonplace remark.)

Refectory, Miss G. A day before the dream I had *lunched* with *Miss G.* in the *refectory*. As I got up to leave the table, a colleague who is in my own department stopped me and said, "Is it true that X. is leaving?" (X. is another colleague in my department.) I answered "Yes," and he said, "I wonder *whom we shall get next*; the men in that post have always been nice fellows." I immediately thought of my friend F., who had preceded X., and is now dead, and I said no more, but hurried away, as the memory was painful to me.

The apparent inciting cause of the dream was a trivial event from the dream-day, in which the centre of interest was *my own coat*. On the night of the dream, a friend returned unexpectedly from South Africa, and dined with me. From my house he telephoned to some other friends, who replied, "Come along at once, and bring Pear, too." On my arrival at their house I was slightly embarrassed at finding that I had forgotten to change my coat, and was wearing a very old torn coat which I was fond of wearing in my study. The incident, however,

probably made more impression upon me than I would admit to myself at the time, for although I knew that my hosts would not resent my unusual dress, and laughed at myself for entertaining such a thought, I was not comfortable all through the evening, and the thought of my coat kept recurring to me. This is one reason for its prominence in the dream; the other one will appear later.

Blazer, with two shields, maroon. At this time, I knew only one blazer with two shields on its pocket; a black one which was habitually worn by F. when he lived with me. He occupied the rooms which I have now—the attic bedroom and the study in which I was when my friend, on the dream-night, took me away in my old coat. F. was also fond of wearing a comfortable coat in his study—the blazer I have referred to; and the many intimate talks we had in this room are, I believe, symbolized in the dream by the blazer which he wore on these occasions. This blazer, however, was not maroon, but black. Maroon is the colour of a Manchester University blazer, which was seen first at the University sports, to which I was taken, on that occasion, by F.

F., Miss G. A few days before the dream, S. and I had called on Miss G., who had introduced us to a Mrs F. This lady has the same name as F., and it should be emphasised that the name is not a common one; in fact I know personally only three people of this name. On leaving, S. had remarked to me that there was a striking resemblance between the faces of Mrs F. and F., especially about the eyes. This resemblance, together with the identity of their names, had also struck me before it was emphasised by S. (It should be remembered that S., too, is responsible for the association between the ideas of *coat* and *Miss G.*) Mrs F. had interested us very much by talking to us about South Africa. The only other person who had lately discussed this subject with me was the friend who was responsible for the prominence of my *coat* in my mind on the dream-night.

Attic, F. As mentioned above, there is an association between my attic bedroom and F., who occupied it before me.

The interpretation of the dream. One or two more remarks concerning some experiences in the waking state which have obvious reference to this dream will, I believe, prepare the way for a very probable interpretation. F. died at a time when I had no opportunity of talking about him to others, as I was then staying with people who did not know him. When I came back to the University, little was said to me about the sad event, for very natural reasons, and thus there had been no chance to share my sorrow with others. But from time to

time I was astonished by the fact that occasionally I forgot momentarily that F. was dead. Once, while immersed in reading, I found a new theory which would have interested him, and was astonished to find that I had begun to write a postcard to him, to call his attention to the fact. My belief is that I had persistently repressed the memory of his death. There was no possible outlet for my sorrow at the time when this painful news reached me, and, later, the feeling of others that little good would be done by talking to me about my late friend closed all possibility of effective reaction to the sorrow. On the dream-day, however, an indirect reference to him was made in conversation, and I hurried away in order to avoid the subject. But the words of my colleague must be remembered—"I wonder whom we shall get next; the men in that post have always been nice fellows."

At the time of this speculation concerning X.'s successor, X. had begun to give me *valuable help in my laboratory*. F.'s help in, and sympathy with, my work was a feature of our friendship which I always remember with the greatest pleasure. In the dream *a colleague is helping me in my laboratory*, and I believe that the meaning of the dream is that F. returns to his post.

I believe that Miss G. represents F., for she introduced me to Mrs F. who at once recalled him, not only to my mind, but also to S. who, by mentioning the resemblance, emphasised it in my memory.

She signals the end of work and the beginning of social intercourse by putting on F.'s coat, just as he used to do.

In the dream she is a colleague in my department, as he was.

On two occasions S. has made an association in my mind with Miss G.; once with F.'s name, once with the idea of 'coat.'

The processes of condensation, distortion and symbolism may be clearly seen here. The scene of the dream is an obvious condensation of the bedroom successively occupied by F. and myself, the laboratory, and the attic which I hoped would form a laboratory in the future. The blazer is composed of two such coats, and it should be noted that even the incorrect colour is taken from a memory for which F. was responsible. Without laying oneself open to the charge of uncritically accepting Freud's theory of the distortion which is brought about in order to pass the 'censor,' it may be pointed out that, had the blazer appeared in the dream with all its characteristics correct, it might have been recognised as belonging to F.

The symbolism, by means of which my friend, although not appearing in the dream, is represented by the most characteristic feature of his

dress, is simple and clear. The dramatization in the dream speaks for itself. The superficial associations which arose from the chance resemblance of two persons, coupled with the coincidence of the identity of their names, are just the material which we should expect, if Freud's theory be correct, to form the core of the dream. Lastly, the symptomatic action which happened in waking life¹ forms another powerful piece of evidence that I had repressed the memory of my friend's death.

In this dream two wishes are fulfilled which were conscious and fully recognised by me in the preceding waking state. Miss G. and I become colleagues in the same department and the attic becomes my laboratory. Behind both these wishes there was a relatively great driving force; the first wish-fulfilment represents a gratifying increase in the number of students in my department and the second an increase in the laboratory accommodation. These wishes are derived from professional and personal interests which are quite clear to me. But it is important to notice the way in which the deeper-lying 'wish,' which in waking life was never overt, but existed probably as a restless, untiring conative tendency, underlies the whole dream.

In view of what Freud has maintained with regard to the action of the 'censor' in waking life, it is important to note, too, the fact that I awoke laughing. The real subject of the dream is one which, had I realised it, would have been connected with an emotion very different from that which I felt on awaking.

Second Dream.

The Apparent Dream. I was in St Ann's Square, Manchester, early in the evening, in the summer. The light was curious; impossible to compare with any light effects seen in waking hours. The whole square seemed to be one large arena (like the arenas used for bullfights), and people were crossing and re-crossing it. At one end of the square (the end opening into Market Street), in the right-hand corner, was a large cinematograph screen, showing moving pictures, and the impression in the dream (which seemed quite natural then), was that the square itself was one vast 'picture-palace.' I was then in position 1 on Fig. 1, uncomfortably close to the screen; i.e. the pictures were not easily seen, and were distorted.

¹ Cf. Freud, *Zur Psychopathologie des Alltagslebens*, Dritte Auflage, Berlin, 1910; Jones, "The Psychopathology of Everyday Life," *Amer. J. of Psychol.* 1911.

Suddenly I found myself in position 2 in the square. The scene had narrowed down to the size of an ordinary room, about 12 feet square, though I could see no walls. The light was brighter, but not very bright, and I recognised it as coming from electric incandescent lamps. I was still in the square, yet people in evening dress were passing and re-passing me, through a partition like a screen. It was, except for the feeling of not being 'walled in,' exactly like being at a University soirée¹, for Professor and Mrs S., in evening dress, passed through the partition and greeted me.

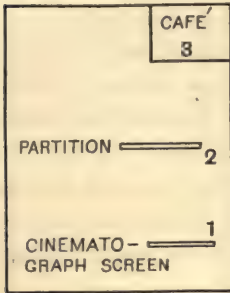


FIG. 1.

I found myself then at position 3. Here the light was dimmer, and I was sitting at a long form, amongst several other forms. People were eating and drinking, and the place seemed like a South German café. The 'Gemütlichkeit' was very apparent to me. (This feeling-tone, and the eating and drinking, were the only 'café-signs,' yet they were quite adequate to complete the perception of the place as a café².) At once Dr Myers walked into the café, sat down by me with a casual greeting,

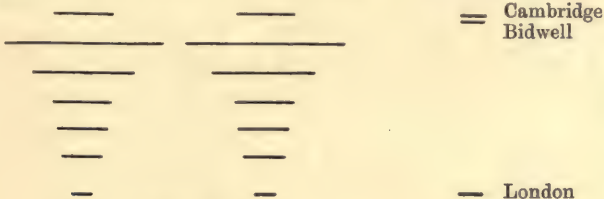


FIG. 2.

FIG. 3.

and took out several sheets of paper. (The impression was that we had both been working in the same laboratory, and had seen each other quite recently.) He began immediately to explain to me that he was beginning a research on 'physical and metaphysical logarithms.' (The work was quite full of meaning and comprehensible to me at the time, and the problem seemed quite familiar.) His first paper contained complex algebraical problems in which two problems were worked out in very neat parallel lines³, in Dr Myers's handwriting, side by side, like the

¹ The consciousness of the 'meaning' of the scene was quite clear, although the 'scenery' would not have suggested a soirée to anyone in the waking state.

² Cf. preceding footnote.

³ These lines were longest at the top and gradually decreased in length. See Fig. 2. I sketched them immediately on awaking, but do not think I read them in the dream.

creditor-debtor columns in balance-sheets (see Fig. 2). I cannot remember if I understood them. The reasoning did not seem difficult. Then he began to draw, on another sheet of paper, a map¹ to illustrate his remarks, which were, "You (meaning the dreamer), go down to London through (or from) Cambridge, and you get short-circuited at Bidwell, on account of the suffrage question." I quite understood this at the time. While he said this, a man bent over both of us. He had the general appearance of a doctor (he wore a morning coat and dark trousers), but was unshaven, and this fact was very unpleasant to me. He kept on interrupting Dr Myers and laughing at both of us. Dr Myers was quite friendly with him, but I was annoyed and irritated at the interruption. (Awoke here.)

Sources of the Apparent Dream.

St Ann's Square, Cinematograph. Before going from Manchester to Cambridge, where I had stayed with Dr M. on July 13th, 1912, ten days before the dream, two business visits had to be paid on the afternoon of the 12th, and the limited time available for them had caused some excitement and interest in the events. The first visit was to my tailor, whom I wanted to remind to send me a suit of clothes to take with me to Cambridge next day. The second visit was to see a sound-proof *partition* in a warehouse. This visit interested me greatly, as, if its sound-resisting qualities proved satisfactory, this type of partition would be erected in my laboratory. On my way between the warehouse and a return visit to the tailor's I met two men carrying an advertisement which announced that a '*picture-palace*' was offering free *refreshments* to its patrons. I had recently visited several picture-palaces, and had discussed them with my father.

The *cinematograph screen* in the dream occupies the same position in the *square* that my tailor's shop does in reality. (The advertisement and the tailor's shop were seen a few minutes after each other.) The actual screen and the unpleasant proximity of it are recollections from an experience on June 29th, when, in a cinematograph theatre which my father and I visited, there were no seats available, except some directly under the screen. The increased flicker and the *unusual angular appearance of the figures* were irritating to us, especially as

³ The 'map' was really a rough diagram which I have drawn as I saw it in the dream (see Fig. 3). The names did not appear on the 'map,' but I understood that they referred to the places marked on it.

the pictures were interesting. I felt some responsibility for the inconvenience to my father, as I had suggested this particular theatre. There was some interest to me in the fact of the increased flicker, and its connexion with the unusually great visual angle subtended by the pictures, also in the one-sided appearance of the flat human figures.

Why does the cinematograph screen appear in St Ann's Square? In St. *Peter's* Square, Manchester (the only other square in the town which, so far as I know, is named after a saint), there is actually a *lantern screen*, upon which changing advertisements are *projected at night*. I have often waited here for the tramcar, and have found the pictures a welcome means of passing the time. We had waited in this way on coming from the cinematograph theatre described above. (See figures 6 and 7, page 301.)

The connexion of *cinematograph—tailor—partition* will now be clear. In position 2 on the map of the 'dream-square' the partition actually appears.

As it happens, the only other member of the staff who is erecting partitions of the same kind as my own, and in the same corridor as mine, is *Prof. S.* Also, the carpenter who was awaiting orders to proceed with my partitions had been entrusted with the task of making a *lantern screen* for me, to be fixed in the *partitioned corridor*.

Partition—Incandescent electric lamps. I had been compelled to postpone giving orders to the electrician about the lighting of the partitioned corridor, owing to the rush on July 13th, although I had wished to do this before going to Cambridge.

Incandescent electric lamps—Soirée—Prof. S.—Bidwell. The last time that I had worked by electric light (the dream took place in the summer), was at a medical *soirée*, a few weeks before the dream. I had had some trouble with the *electric bulb* above my apparatus. This apparatus had been arranged in such a way as to leave room for an exhibit by *Prof. S.* The failure of the light, and its insufficiency when attended to, were annoying to me, because we were carrying out *Bidwell's* colour experiments, which need bright illumination. These demonstrations had excited much interest and questioning.

Lighted-up partition—Refreshments. The association given above (page 294) partly accounts for this, but the laboratory used at the *soirée* (see above) opened into the *refreshment room*, into which we had gone when our experiments failed¹.

¹ In St Ann's Square there is actually a *café*, which I frequently visit, in position 3 in Fig. 1.

Forms—Bidwell—Dr Myers—Café. Before leaving Cambridge, the last two subjects I had discussed with *Dr Myers* (on the station at Cambridge) were the questions of *sound-proof partitions* and *the writings of Bidwell*. At Cambridge, too, another psychologist had spoken to Dr M. of the habit of the *psychologists* at a *German* university, at which we had both studied, of *discussing, and working out, the results of their experiments in the café opposite the laboratory*. Not long before this dream (I believe, the day before), I had mentioned the same fact to my father. At that time I was working in a large room, and had arranged my books, including those dealing with *Bidwell's* work, on a table which was surrounded by *long forms*. This room would, for the next few weeks, represent my work. (Being a habitual visualizer, I frequently represent to myself a whole side of my activities by a visual image of one important thing connected with it.)

The connexion between *Bidwell, refreshments* and *café* therefore seems obvious. There is, however, still another reason for their close connexion, which will appear at the end of this explanation.

Dr Myers—Logarithms—Lines. I had remarked to Dr M. in our conversation on the station that there seemed to be a probability that a colour effect which Bidwell could not understand (reported in his paper in the *Proceedings* of the Royal Society), was simply the violet in 'Fechner's colours.' Professor Alexander has said more than once to me that there seems to be a more intimate connexion between Fechner's Law and the general nature of *logarithms* than has hitherto been supposed. He has also lent me a typed sheet of manuscript which deals with the question, and in which, I remember, there is mention of the '*physical-psychical* relation,' and of the *metaphysical* concepts necessary in his treatment of the subject. I remember saying to him, "I think I understand it." The lines in Dr M.'s MSS. seemed in the dream to be parallel, but the actual figures which formed them were not clear in the dream. The most striking feature on the paper was the fact that there were *parallel lines*.

The last time that Dr M. was in the Manchester laboratory, when he was discussing the 'partition' question with me, we discussed also the 'Bidwell' work in connexion with the familiar phenomena of Benham's disc, in which black lines on a white ground (Fig. 4) appear coloured when the disc is slowly revolved. As I revolved the disc, he drew for me, on a sheet of paper, the lines of the top, writing by the side of them the colours that he saw (Fig. 5). I was able to find the actual paper, of which Fig. 5 is a reproduction. These lines, longest

at the top and gradually decreasing in length, are just like the lines of the calculation seen in the dream. This paper was found filed with those relating to the 'Bidwell' work which lay on the table amongst the long forms.



FIG. 4.

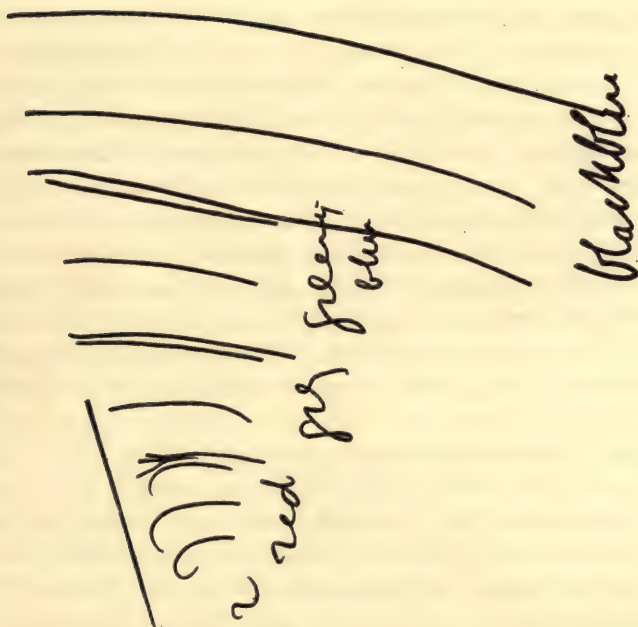


FIG. 5.

Cambridge—Bidwell—London—Suffrage. A few days before the dream I had looked at the *road map* of the route between Wisbech, in Cambridgeshire (near which town I was then staying), and *London, via Cambridge*. I had noticed Great Shelford, where I had stayed with Dr M., but the study of the map had taught me nothing new, and I had noticed the relative positions of Wisbech, Cambridge and Shelford simply because they had interested me.

The last time that I had visited Dr M. at Shelford was at a time when I had intended to go to London from Wisbech, and he had invited me to take Cambridge and Shelford on the way. Dr M. had also said in my hearing, when he was at Prof. Alexander's house in Manchester, to a lady who had asked him about his views on the question of women's suffrage, "*You should come down to Cambridge.*"

Until several hours after the dream the reason for the substitution of *Bidwell* for *Shelford* did not occur to me. I may have noticed before that Shelford Bidwell is the full name of the investigator who had occupied my thoughts, but 'Bidwell' seemed, in the dream, to be quite the natural name of the village. The *position of Bidwell on the 'dream-map'* was undoubtedly that of Shelford, for it was understood to be three or four miles south of Cambridge, or rather, in the terms of the dream, so many miles nearer London, on the way from Cambridge.

Short-circuited. While, during my stay at Cambridge before the dream, I was asking a question about Benham's disc, an American colour investigator came into the room. He apologized for coming in late, and explained to us, "I've been side-tracked," meaning that he had lost his way. Americanisms in psychology being, owing to James' and Titchener's influence, especially interesting to me, I often use them in my own thinking, and the words 'side-track' and 'short-circuit' are frequently used in the same sentence when dealing with the psychology of the thought processes, *e.g.* thoughts are 'side-tracked' or 'short-circuited.' The American above had struck me at the time as being '*very American*,' and I was amused at his use of the word 'side-tracked.'

The key to the whole dream, however, is given by

The man who interrupted. He is not actually a doctor, but is intimately connected with medical work, and dresses just as he is dressed in the dream. He is very dark and clean-shaven (but I have never seen him unshaven, though his chin is very dark). The dark chin has become, in the dream, an unshaven chin. The smile he wore in the dream is the one which is, in actual fact, a distinctive feature of him.

The man who interrupted—the German café. See the interpretation of the dream.

The interpretation of the dream seems to me to be conditioned by the following facts. Before going to sleep, and during the day before the dream, I had been planning my work for the Long Vacation. This work, as I had been thinking about it, was represented visually by an

image of the work table spread with books and files. (I often use such an image as a 'scheme' in thinking; cf. the map in the dream.) My immediate interests were in the colour work (*i.e.* the 'Bidwell' work), on which I wanted to begin at once (partly because brilliant sunshine was available at the time), and for which I had prepared some apparatus. But on the day before the dream¹ I had made a decision to leave this work alone for a time, and to begin to attack a problem concerned with Memory. The reason for this was that I had recently been asked for advice on this point by the '*medical*' man in the dream. In fulfilment of a promise I had made him (made after the necessary work had been planned with him over a *German dinner* in the *German restaurant* of the Midland Hotel, Manchester, which occupies the same position in St Peter's Square (the square which actually contains the lantern screen) as that of my café in the dream-square (compare Figs. 7 and 1)), I had decided to begin the 'Memory' work before the 'Bidwell' work. The data for this memory work I had obtained from this man who, in the dream, 'kept on interrupting' Dr M. These data actually lie in the memory file on one side of my work table, Dr M.'s data in the 'Bidwell' file on the other.

The meaning of the dream seems to be clear. On the 'dream-day' I had actually decided (finally, as I supposed) to 'shelve' the 'Bidwell' work, although my mind was full of it, and to attack the 'Memory' work at once, because of my promise. The dream throws valuable light on the striving of impulses which may still go on, even after an apparently final decision has been arrived at in waking consciousness. In the waking state the conflict was brief, and apparently decisive. The dream re-opens it in a characteristically vivid manner.

It is in a dream of such richness and complexity as this that one may fairly seek for confirmation or negation of the existence of Freud's alleged dream mechanisms—those processes which combine to form what he calls the dream-work—the distortion of the latent thoughts into the apparent dream. Let us, therefore, carefully examine the material of the above dream.

In the first place, the dramatization in this case is well-nigh perfect. The dream turns this mental conflict, which in a waking state would have been one of mere thought (accompanied no doubt by some imagery), into a kind of problem play, in which the two opposed influences become human beings. In the dream, the attractive 'Bidwell' work is

¹ The 'dream-day.'

represented—for the many reasons given above—by Dr M.; the less attractive but urgent ‘memory’ work by the man who ‘keeps on interrupting’ him. My thoughts of the ‘Bidwell’ work on the dream-day had been continually interrupted by the thoughts of the ‘memory’ work, until I had put an end to this state of things by deciding to postpone the former work. In the dream I am annoyed and irritated by the *representative* of the ‘memory’ work, who persists in interrupting the *representative* of the ‘Bidwell’ work.

It should be noted that in the dream I am not irritated at the *real* cause for annoyance, viz. the work which I have promised to do, but at the man to whom I have given my promise—a man with whom I have always been on friendly terms. Moreover, the dream seizes upon one harmless feature of the man—his dark chin—to transform it into a feature which is very unpleasant to me—an unshaven chin. Freud’s assertion that the emotional tone which is attached to a thought in the latent content appears in the dream attached to another, related, element which is not under the ban of the censure, must be considered in connexion with this feature of the dream. It is quite true that professional and scientific interests would oppose, in waking life, a strong resistance to the temptation, which probably arose here, to consider the *work itself* as irritating. As a matter of fact, the work was very interesting to me, but the fact that my mind was full of the newer problem had been sufficient to displace it temporarily from the focus of my interest. Here the emotional tone seems to be displaced from the work to the man, and in particular to one feature of him.

The condensation employed in fashioning the ‘stage’ and the ‘scenery’ of this dream is clearly visible. The most striking case is that in which the two squares, the four restaurants¹, the two lantern screens and the two laboratories fuse to form the scene of the events. (A comparison of Figs. 6 and 7² with Fig. 1 shows this clearly.) The manuscript paper is a fusion of two papers, and the speech is a clear condensation.

The kind of superficial association involved in the play on words in the names Shelford and Bidwell, utilising a coincidence, is again a very common factor in the dream-work of Freud’s theory.

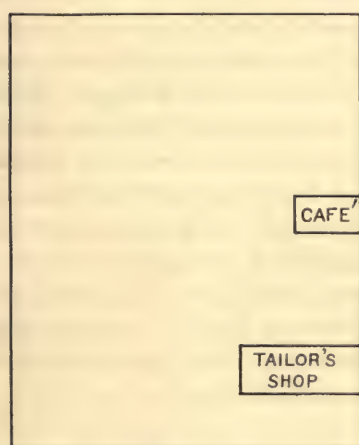
It seems undoubtedly true, then, that several of the processes which,

¹ The German restaurant at the Midland Hotel, the café in Germany, the café in St Ann’s Square and the refreshment room at the soirée.

² Actual plans of the relevant details, with their relative positions, in St Ann’s Square and St Peter’s Square respectively.

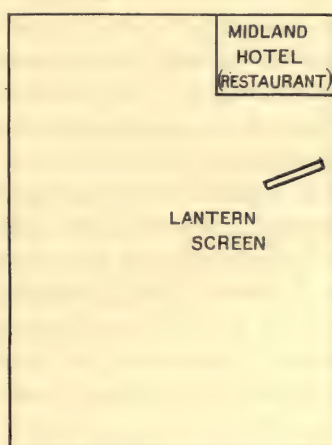
according to Freud, are characteristic of the dream-work, are illustrated in this dream.

What, then, can we regard as the *meaning* of the above dream? We may, I think, fairly describe it as the dramatic representation of a mental conflict in which the opposed conative tendencies at work appear in disguised forms. We must note, however, that I awoke before I was able to see if the interrupter was completely successful. And here we may ask, "What of the wish theory?" It may be that my wish to pursue the interesting work was actually being fulfilled in the dream when the waking consciousness, gradually regaining its power, introduced the counter-thought of my promise, and that this, depicting itself as dramatically as the first thought, appeared as the 'man who inter-



ST. ANN'S SQUARE

FIG. 6.



ST. PETER'S SQUARE

FIG. 7.

rupted.' In connexion with this it must be remembered that it was only at the end of the dream, just before I awoke, that he appeared. I was able to find no cause for my awaking, such as a noise or a sudden change of illumination. It should be noted, too, that the partition, lighted up with incandescent lamps, represents the state of affairs as I should wish it to be, and that at the soirée in the dream the lamps are efficient, while in the event which caused this dream-episode they were unsatisfactory.

The phrase, "You get short-circuited at Bidwell" is interesting. Firstly, it strings together the words from several experiences to form a new sentence, and is obviously a condensation; secondly, it expresses in

a figurative way the new direction given to my thoughts by the 'Bidwell' work. It seems possible that this expression contains the incipient form of what in a more highly developed state might have been a combination of two rather 'cheap' forms of wit—the play on words in using 'Bidwell' for Shelford, and the use of an actually heard phrase in a new way¹.

Another fact in connexion with this might be mentioned here, viz. that Freud's theory of the short-circuiting or side-tracking of emotional interests was known to me, through American writers, at the time when I dreamt this dream. I do not wish to assert that I believe strongly that the sentence expresses more than the 'short-circuiting' of my thoughts by the 'Bidwell' work, but the completion of the sentence should be noted—"on account of the suffrage question." It should be remembered that one of the chief arguments used by the opponents of Women's Suffrage is that they believe that certain highly important interests may be "short-circuited on account of the suffrage" should women take too intense an interest in public affairs. The existence of this argument was well known to me at this period, and the psychological aspect of this controversy was at that time, for several reasons, frequently in my thoughts. (But this possible interpretation occurred to me at a time much later than that at which I analysed the dream, so that I wish it to be considered quite separately from the facts given above. I have no proof that such a thought entered into the composition of the dream, but from a consideration of my special interests at the time of the dream, I am inclined to think it probable.)

V. CONCLUSIONS.

The two dreams which have been analysed in this article illustrate clearly the processes which Freud has termed the 'dream-work.' In them we find instances of dramatization, symbolism, condensation, displacement, and the superficial association which, in both dreams, makes use of a chance identity of the names occurring in two past experiences.

The distortion in the dream seems to bear traces of evident purpose, and its character, when taken in conjunction with the events in waking life which are represented in the dream, supports the concept of a 'censorship' which is evaded by it.

¹ Cf. Freud, *Der Witz und seiner Beziehung zum Unbewussten*, Wien, 1905.

In the first dream, although it presents the fulfilment of two conscious wishes, the important underlying wish which was fulfilled was unconscious. Though in one sense this wish was of an infantile character, it can scarcely be said to have emanated (at least in its present form) from childhood.

In the second dream, the wishes which gave rise to it are clearly seen on analysis, but there is no trace, even on careful psycho-analysis, of an unconscious wish. The tendency suppressed in the waking life was a conscious one.

On the whole, these dreams appear to support many of the main assertions made by Freud. They do not, however, afford evidence for his conjecture that the infantile and unconscious wish is a *necessary* cause of the dream. The consideration of many other dreams of my own, and of other ordinarily healthy persons, leads to the opinion that this extension is a generalisation supported by insufficient evidence. Freud cannot be said to have shown that his theory is valid for *all* dreams, for the dreams which are recorded when the dreamer is awakened in the midst of deep sleep, as well as those which occur immediately after falling asleep, have not yet been subjected to detailed study combined with an examination of their latent content. Neither has he proved the general validity of his theory for the dreams which remain in consciousness on awaking, although many such dreams are explicable by means of his hypothesis.

It is possible that future work may result in the further analysis of the 'dream-work' and the 'censorship,' and that they may be shown to depend upon factors with which we are already familiar. But there seems little doubt that such processes exist and play an important part in mental life, and that Freud's striking demonstration of them is a valuable contribution to psychology.

(Manuscript received 6 December, 1913.)

THE CONDITIONS OF BELIEF IN IMMATURE MINDS¹.

By CARVETH READ.

- § 1. *Introduction.*
- § 2. *The first ground of belief is perception.*
- § 3. *Classification of secondary grounds and causes.*
- § 4. *Immature minds perceive as we do.*
- § 5. *The imaginative beliefs of savages are moulded by passion and custom and are allied to play-belief.*
- § 6. *Effects of intense imagination, absence of criteria, and mental incoordination.*
- § 7. *The ratiocination of immature minds.*
- § 8. *The weakness and gradual decadence of imaginative beliefs.*
- § 9. *The utility of imaginative beliefs.*
- § 10. *Black magic and red religion.*
- § 11. *Imaginative beliefs and scientific ideas.*

§ 1. INTRODUCTION.

THE expression 'immature minds' is here used to include children and backward adults. Backward adults include the great majority of all civilised nations and a still greater proportion of barbarians and of savages. But the present essay is chiefly concerned with savages; because amongst them the conditions of belief to be examined operate most intensely and with least qualification or restriction. And the word 'savage' is used, not offensively, but for the sake of brevity, to denote the more backward peoples, and as less misleading than 'primitives'; since no people is known to us that must not have had a history as long as our own.

¹ An abstract of this paper was read at the meeting of the British Association for the Advancement of Science, Birmingham, Sept. 1913.

The general conditions of belief have so often been discussed that I must ask to be excused for restating them in the way most convenient for the following discussion¹. It seems to me that they are the same for all minds, except that (1) their relative influence varies at different stages of development, and that (2) it is only for some minds amongst civilised peoples that there exists a Logic or Methodology: definite categories of judgment (such as 'cause' and 'quantity') and canons of evidence, without whose sanction they do not fully believe anything—at least, within a certain sphere of investigation or special study; though, outside of it, they may be credulous enough. Of course, our Logic may be faulty, or we may make mistakes in applying it, and so adopt erroneous beliefs in spite of our care.

§ 2. THE FIRST GROUND OF BELIEF IS PERCEPTION.

The ground of all belief is perception, directly or indirectly; and in perception certain sensations have a certain order of prepotency;—pain, tacto-kinesthesia, vision or smell, etc. This is common to men and animals; and is such a matter of course that we are apt to overlook its significance and necessity. Belief has sometimes been discussed as if it were chiefly concerned with relations of ideas; and systems of philosophy have sought justification in the coherence of their ideas, with little or no regard (not to say with contempt) for the coherence of ideas with perceptions. But nearly the whole of every man's life (savage or philosopher) passes in an attitude of unquestioning belief in the evidence of his senses.

Methodologically we know that perception is fallible; but in the long run it overrules everything else; and experimental methods consist in taking precautions against the errors of perception, and in bringing every hypothesis to the test of perception.

§ 3. CLASSIFICATION OF SECONDARY GROUNDS AND CAUSES.

Further grounds and causes of belief are divisible into (1) the evidentiary, which (though often misleading) may generally be justified by reflection as raising some degree of probability; and (2) non-evidentiary, which (though very influential) cannot be justified by reflection as having any logical value.

¹ Perhaps indebtedness should be acknowledged especially to my old friend Prof. James Sully. See *The Human Mind*, c. xii. and *Sensation and Intuition*, Ess. iv.

306 *The Conditions of Belief in Immature Minds*

(1) Evidentiary grounds of belief are (a) memory, shared by the higher animals and indispensable to ourselves; (b) testimony, which must be trusted if language is not to be useless: both of these grounds are supposed to rest upon previous perception. And (c) inference, also shared by some of the higher animals, and necessary to all original adjustment to the future or to unperceived circumstances, but highly fallible, and constituting the chief problem for the exercise of Logic when that science arises: especially to distinguish, amongst inferences, valid illations from merely verbal substitutions and from incoherent imaginative analogies. Incoherent imaginative analogies become, with the growth of reflection, relegated to the region of play-beliefs and poetry; but where, in the immature mind, necessary beliefs and play-beliefs are very imperfectly differentiated, such analogies are effective in practical affairs.

Testimony gathers force with the numbers and consideration of those who support it, and especially with their unanimity.

As the constructive instinct deals with beliefs and play-beliefs, they are piled up into systems of Science, Theology, Philosophy, Astrology etc.: in which systems, each belief strengthens, and is strengthened by, the rest. Even without systematization, the mere structural similarity of beliefs formed upon the same implicit principles of causation, or of magic, or of animism, throws them into apperceptive masses; and such systems or masses readily assimilate and confirm new inferences having the same character, and offer resistance to all inferences having a different structure.

(2) The non-evidentiary causes of belief are chiefly the following:

(a) The agreeableness or disagreeableness of any judgment draws attention to, or diverts it from, such a judgment and the evidence for it: except that some disagreeable feelings, especially fear, by a sort of fascination of attention, are favourable to belief in the imagined danger.

(b) Every desire fixes attention upon beliefs favourable to it and upon any evidence for them, and diverts attention from conflicting beliefs and considerations. Thus every desire readily forms about itself a relatively isolated mass of beliefs, which resists comparison and, therefore (as Ribot says), does not recognise the principle of contradiction. Incompatible desires may be cherished without our becoming fully aware of their incompatibility; or if the fact obtrudes itself upon us, we repudiate it, and turn away.

The more immature a mind, and the less knowledge it has, the less inhibition of desire is exerted by foresight of consequences that ought

to awaken conflicting desires; the less compassion one has, the less is desire inhibited by its consequences to others: therefore the less check there is upon belief.

(c) Voluntary action in connexion with any belief, whether of a rational kind or in connexion with rites and ceremonies, favours that belief: (i) by establishing the idea-circuit of means and end, the end suggesting the means to it, and the thought of means running forward to the end; a circuit that resists interruption: (ii) by the general effect of habit and prejudice; for every habit of action or of thought has inertia and, moreover, it is agreeable, and to break it disagreeable; so that, again, a relatively isolated system is formed, which resists comparison and criticism.

On the influence of desire and rites etc. depends the "will to believe." We cannot believe anything by directly willing it; but we can will what to attend to, or what to do.

(d) Finally, belief is determined by certain social influences other than testimony and tradition: especially by sympathy and antipathy between families, parties, tribes; and by imitativeness and suggestibility (qualified fortunately by contra-suggestibility); so that beliefs become fashionable, endemic, coercive, impassioned and intolerant.

§ 4. IMMATURE MINDS PERCEIVE AS WE DO.

All these grounds and causes of belief, evidentiary and non-evidentiary, are common to both civilised and immature minds; but their proportional influence is very different at different stages of development; and in immature minds the power of the non-evidentiary causes is excessive.

Perception, in normal circumstances, is accepted by all as a matter of course: it controls the necessary activities of practical life in hunting and in industry, in making weapons, hoeing the ground, building houses, etc.; however these processes may be modified, or interrupted, by the intrusion of beliefs derived from other sources. If the savage sings a spell to his prey, or his weapon, or tool, or buries a slave under his house, he may thereby increase his own confidence in the work; but, otherwise, if it be no better, neither need it be the worse for such hocus-pocus. The properties of matter exact practical observance, without which nothing can be done. Even magical practices presuppose a sane perception of the central facts: as who is acting, for what purpose, when and where, with what and with whom. Upon this

basis there may be an astonishing accretion of imaginative belief; but we shall see that there are limits to the effectiveness of such beliefs.

M. Lévy-Bruhl, indeed, in his very interesting book, *Les Fonctions Mentales dans les Sociétés Inférieures*, maintains that, under the influence of social ideas (*représentations collectives*), the primitive mind actually perceives things differently from what we do. Whilst we succeed in attaining an objective presentation, eliminating subjective associations, for primitives *propriétés mystiques, forces occultes* are integral qualities of the object. He grants that in certain cases of immediate practical interest, we find them very attentive and able to discriminate slight impressions, and to recognise the external signs of an object on which their subsistence or even their life depends (p. 40); but in the *très grande majorité des cas*, their perceptions are over-weighted by subjective elements. This doctrine reverses (I venture to think) the real relations between perception and other causes of belief and their proportionate influence in savage life. It is not only where subsistence or life is at stake that backward people see things as they are: in merely experimental tests, Dr Rivers found amongst both Papuans and Todas, that, as to suggestibility in perception, they showed a high degree of independence of judgment¹.

§ 5. THE IMAGINATIVE BELIEFS OF SAVAGES ARE MOULDED BY PASSION AND CUSTOM AND ARE ALLIED TO PLAY-BELIEF.

The peculiarity of savage beliefs, then, is not due to corrupt and clouded perception, but to the riot of imagination, unrestrained by criticism and reinforced by the popular consensus. The savage's imagination is excited by the pressing needs of his life in hunting, love, war, agriculture, and therefore by love, hate and grief, by fear, suspicion and anxiety. Imaginations spring up in his mind by analogy with experience; but often by remote or absurd analogies; and there is no logic at hand to distinguish the wildest imaginative analogies from trustworthy conclusions. The same pressing needs and the same emotional storms affect a whole tribe and stimulate everyone's imagination; and, tracing its origin (no doubt) from ancient times, and slowly clearing and solidifying, there grows up a mass of public imaginative beliefs, which are inculcated into every individual by tradition, suggestion, imitation, sympathy. Such beliefs are embodied in rites, ceremonies, formulae; and are, in fact, customs. They have, therefore, the

¹ This *Journal*, Oct. 1905, 393.

strength of custom in the habits of individuals and families and in public respect; and the weakness of custom, inasmuch as the observances may continue whilst the beliefs are forgotten, or may decay and disintegrate by social fatigue and neglect. In their flourishing period they extensively modify the behaviour of tribesmen in all practical affairs, sometimes helpfully or harmlessly, sometimes injuriously and destructively. In general, imaginations are prevented from modifying a tribe's conduct beyond certain limits by biological necessities; but exceptionally they result in tribal insanity, tending toward, if not accomplishing, the tribe's destruction, as in extreme cases of the practice of human sacrifice, or of the ordeal by poison.

Indeed, so violent and tyrannous is the effect of superstitious beliefs in many cases, that it may be difficult to understand how they are almost entirely born of the imagination: the qualification 'almost' will be discussed presently with reference to Magic (§ 8). In a civilised country there are always current some beliefs as imaginative as any to be found in the middle of Africa; but whenever the imaginative character of a belief has been recognised, we class it as 'make-believe' or (better) 'play-belief,' and it passes into the region of fine art, fiction, sports and pastimes. If such things have any place in our life, we go to them of personal choice in the intervals of business; are fully aware, for the most part, that the matter which absorbs our thoughts for the time is not really important—whether the hero or villain will prosper, whether the fox will be killed or get away; and perhaps excuse our condescension on the ground that the tone of fiction affects public morals, that fox-hunting maintains the breed of horses, and that (at any rate) the bow must not be always bent. Under the influence of the fine arts and literature our emotional states may be intense; but they are dissociated from action, exist for their own sake, have a special tone and require only an imaginary satisfaction. With a backward people a much smaller portion of their imaginative possessions and pursuits has been differentiated as play, and much that seems to us absurd seems to them necessary; the actions and observances that express their beliefs are not performed as a matter of personal choice, but of public custom: the ends to be obtained (they think) are the same as those of what we call business.

To understand how, in spite of these contrasts, the magical or religious beliefs of savages and the play-beliefs of civilised men, having a common ground in imagination, are closely allied, we must call to mind the many degrees of intensity of play-belief in ourselves, varying

from the momentary entertainment of playing with a child, through various grades of fiction or ceremony, down to a deeply serious frame of mind, a profound movement of dread or compassion that may long outlast our play. A child's absorption in such beliefs is far more intense than ours; but his circumstances prevent him from attaining to the convictions of a savage. The child of civilised people has little or no support in tradition (except sometimes from nursemaids); he is not driven by the desires and anxieties of subsistence; and he is frequently interrupted by his seniors. The savage has an overwhelming tradition and authority, pressing anxieties, and no seniors. Until the civilised sceptic reaches his shores, there is nothing but tardy experience or social fatigue to check his vagaries. His imagination vies with the sense of reality, often overpowers it; yet (we shall see) his imaginative beliefs show many signs of their insecure foundations.

§ 6. EFFECTS OF INTENSE IMAGINATION, ABSENCE OF CRITERIA, AND MENTAL INCOORDINATION.

It is not only the influence of society and tradition that renders imaginative beliefs coercive to the savage; in the immature mind of the individual there are certain characteristics favourable to their prevalence.

(a) The process of imagination itself, the picture-thinking of savages, seems to be more vivid, sensuous, stable, coercive, more like perception, than our own normally is. "The Australians," say Spencer and Gillen, "have the most wonderful imagination¹." They often die of it. So do Hindoo peasants, Maories, Negroes and others, if they know that they have been cursed or have broken a taboo. Hence there is a tendency to believe in imaginations as perceptions are believed in; and to believe in the efficiency of rites, because the mere performing of them with a purpose makes their purpose seem to be accomplished. When a man of intense and excited imagination makes an image of an enemy, and stabs it, that his enemy may suffer, his action gratifies the impulse to stab with its associated ideas, as if he actually wounded the enemy himself; and so revenge seems to be a present fact.

The same intensity of imagination is found in civilised children, and is greater than in ordinary adults². Savages, too, seem to dream more vividly and convincingly than is usual amongst ourselves; and

¹ *Native Tribes of Central Australia*, p. 462.

² Cf. R. R. Rusk, *This Journal*, Dec, 1910, v.

they are said to be more liable to hallucinations. Their dreams are attributed by travellers sometimes to fasting, voluntary or involuntary, sometimes to extreme repletion when there is an opportunity for it. Physiological conditions of the immature brain may explain the intensity of imagination, the vividness of dreams and the hallucinations.

(b) But more important than any intensity of picture-thinking to the growth and persistence of imaginative beliefs, is the absence of a standard by which they might be discredited. One reason why we believe our memories, and not our imaginations, is that, whilst in both cases the images (or elements of images) are derived from experience, in memory the relations of images in place, time, and context are also derived directly from experience; whereas, in imagination, images or their elements are reconstructed by analogies, often very vague analogies, of experience, or by condensations the most capricious. Hence, to make imaginations credible for us, the relations of experience must be faithfully imitated, as (*e.g.*) in *Robinson Crusoe*. But outside the practical, repetitive, necessary course of life, observation of fact by immature minds is not exact and coherent; and, therefore, their memories are not coherent, especially as to relations of time; so that imaginations suffer little by comparison with such memories. There is not enough orderly memory or general knowledge to discredit even absurd imaginations; for so far as observation and memory are disorderly, generalisation, conscious or unconscious, is impossible. Hence not only traditional myths may be monstrous and arbitrary, but occasional tales of private invention, amongst both children and savages, usually exhibit disconnected transitions and impossible happenings. Yet they satisfy the immature mind.

But even so far as standards of judgment exist amongst savages, derived from repeated experience in their practical life, there are certain conditions of the immature mind that hinder the comparison of ideas and, therefore, the criticism of beliefs.

(c) About every imperative need, such as success in hunting, with its correlative desires and anxieties, rites and ceremonies grow up to gratify imaginatively the desires, and relieve the anxieties; and the ideas of these observances form relatively isolated systems. To us these ideas generally seem absurd and irrelevant, when compared with the facts of the savage's own experience. We see a hunter, for example, endeavour to gain his ends by two distinct series of actions. In one he fasts, enchants his weapons, casts spells upon his expected prey; in the other he carefully prepares his weapons, patiently tracks his prey, warily

approaches, and slays it. The latter series we approve and appreciate as causation; the former we ridicule as hocus-pocus, contributing objectively nothing to the event; and we pity the 'heathen in his blindness.' And, indeed, he may be said to be mind-blind; for in observing the rites, his attention is so occupied with means and end, and caught by the circuit in which these ideas revolve, and he is so earnest in carrying out the prescribed actions, that he cannot compare them with the really effective actions, so as to discover their absurdity and irrelevancy. In short, a state of mental dissociation is established for the system of magical ideas. So far does illusion go, that probably he regards the rites as the most important part of his proceedings. But that is not really his deepest conviction: he trusts in Magic, and keeps his bow-string dry.

(d) In the case of children we may assume, and in the more backward races of men we may suspect, that the comparison of judgments is difficult or, in the more abstract cases, even impossible, because of the imperfect development of the cortex. There must be some structural conditions of the free flow of energy through all organs of the brain, corresponding with the associability and comparison of all ideas. We may doubt if these conditions are complete even in good, cultivated minds; since everybody finds one or another study or art especially difficult for him; or the freeing of himself from this or that sort of prejudice especially repugnant. Such imperfections of structure, greatest at the lowest levels of organization, and gradually decreasing as ideal rationality is approached, we may call incoordination; and, so far as it obtains, the results must be somewhat similar to the discoordination, the breaking down or interruption of organic efficiency, that occurs in hysteria, hypnosis and some forms of insanity.

Effective incoordination may, however, be merely functional, from defect of education, or for want of practice in thinking.

In either case, whether from defect of structure or from high synaptic resistance, there will be failure of comparison and, therefore, of criticism, and also (we may suppose) a greater intensity of imagination and of dreaming and a liability to hallucination, such as is said to be generally the case with immature minds.

§ 7. THE RATIOCINATION OF IMMATURE MINDS.

So far as failure of comparison and criticism occurs there must be an absence of Logic. Our Logic consists of a few universal principles, generally accepted (their full recognition implying the widest comparison of types of judgment), with which any more particular judgment may be compared in order to test its validity. One man may be a great student of Logic and a very inefficient reasoner; another may never have opened a text-book, or even a primer, and yet show, by the definiteness of his judgments and the adequacy of his plans, that he is a sort of incarnate Logic, that his mind works according to reason or (in other words) according to the order of facts. Such men occur amongst backward peoples. The Basuto chief, reported by Casalis in 1861 (quoted by Prof. Haddon in *Magic and Fetichism*), said: "Sorcery only exists in the mouths of those who speak of it. It is no more in the power of a man to kill his fellow by the mere effect of his will, than it would be to raise him from the dead. This is my opinion. Nevertheless, you sorcerers who hear me speak, use moderation." He knew the falsity of the belief, and he also knew the force of the illusion. Congenitally of superior mind, the instructions of experience had brought him thus far; and from his high level down to the region of imbecility there are all grades. The average level is shown by the prevalence of Magic and Animism. How are such beliefs arrived at?

Two accounts of savage reasoning have been given by those who admit that savages reason at all: one is that they reason correctly from absurd premises; the other that they reason absurdly from correct premises. There is (I think) some truth and some error in both these doctrines. For the sake of comparison, let me make bold to take the usual example of the syllogism, slightly altering the way of writing it for reasons that will presently appear:

All men are mortal

||

∴ Socrates is mortal.

The sign of equality, written vertically, marks the minor premise—"Socrates is a man"; that is to say, for the purpose of the argument, he is the same as other men.

It is now (I suppose) admitted that our reasoning, in everyday life, does not take place in this explicit formal way. We do not think first of the major premise and then of the conclusion; we need not think of

314 *The Conditions of Belief in Immature Minds*

the major premise at all. If someone doubts the judgment that Socrates is mortal, and asks for evidence, we may think of the major premise, and then put it into words for the first time. In order that premises may determine our judgments it is not necessary that they should ever have been formulated; as we see in the premise—"Magnitudes equal to the same magnitude are equal," which determined the use of a common measure, the five fingers or the cubit, ages before it was explicitly stated. Premises of this high degree of generality are called 'forms of thought'; but forms of thought are established by experience in very concrete material, either by repetition of experiences constituted by similar relations, or even by a single impressive case; and that 'men are mortal' is one of these forms¹. Such forms determine our judgment by the assimilation of relations, ratiocination, or analogy (in the widest sense of that word); and that 'Socrates is mortal' is a judgment so determined.

As for savages, experience settles also in their minds forms of thought, latent major premises, which determine their judgments, and which, as types of experience, are roughly true. They know that for a man to fall into the fire, or to put his hand into it, hurts him; that when a man dances and sings he feels more energetic; and that he thrives by eating solid food. From such truths they draw inferences by analogy. According to contagious Magic, thus:

Latent premise: To throw a man into the fire hurts him:

||

∴ To throw in a lock of his hair hurts him.

Or, according to mimetic Magic, thus:

||

∴ To throw in his image hurts him.

Of course, as the major premise is not consciously referred to, nor is the minor. Explicit premises are no part of primitive reasoning, which proceeds dynamically upon analogies and, if upon bad analogies, knows not how to check or rectify them. The minor premise especially is an invention of Logicians, because it is necessary to their province, which is not reasoning, but proof.

But how can a man believe that a lock of hair, or nail-parings, or the footprint of his enemy may, for the purpose of his action, be treated

¹ It is often said that savages do not believe that man is naturally mortal; and this seems in many cases to be true. But the belief that man is not *naturally* mortal must not be mistaken for a belief that, in fact, man does not die.

as the enemy himself? Because he passionately desires it; because the substitute intensifies his imagination of the enemy, and the action he performs upon it gives him relief. How can a man believe that the image of his enemy may be substituted for him? Because by conceiving it so (it need not really resemble him), it excites the same reactions and gratifies his rage.

It will be noticed that the famous maxims of Magic, that whatever has been in contact with a man—or that any likeness of a man—may be substituted for him, appear to be grounds of the minor premise. It is not to be supposed that the savage, who seems to act upon these maxims, is explicitly aware of them, or has ever generalised and formulated them. In whatever way they exist in his mind, their derivation seems to be another case of illusory analogy. To injure an integral part of a man injures the man himself; therefore to injure a detached part of him has the same effect. The extension of this inference to include clothes and footprints and its exaggeration of the resulting injury are due to the force of hatred. Again, as a reflection or shadow implies the presence of the man who casts it, so does his picture or image¹.

As to the invisible force, *mana*, or whatever it may be called, pervading things that have once been in contact, and operating at a distance, which many savages undoubtedly believe in, it is easy to point to facts of experience,—light, sound, odour, infection—from which it may have been abstracted. But surely so refined a notion cannot lie at the foundation of Magic: we must begin the explanation with some much simpler mental processes, such as have above been indicated, and which seem to need no further explanation.

Again, there are certain other reasonings implied in savage practices, where the error lies not so much in the minor premise as in the minor term. Thus:

Latent premise: Sense of energy is increased by singing, dancing etc.

||

∴ Magical power is increased by the same means.

Or, according to Animism, thus:

Latent premise: Men eat solid food.

||

∴ Ghosts eat ghostly food.

Here the minor premise seems to us plausible, in the light of savage

¹ Cf. H. G. Spearing, *The Childhood of Art*, 92.

ideas; but the minor terms are imaginary. There are no magical powers and no ghosts; and, therefore, the minor premise is still absurd. In saying that there are no magical powers, I do not mean that the magician has no professional powers, but that such powers as he has are not magical.

It follows from these considerations that in the reasoning of Magic and Animism, for the most part, if stated in our recognised forms, the major premise is empirically true; the minor premise is false: the form of reasoning is the same as ours, in so far as it is analogical in the most general sense (a comparison of relations); but it is invalid, because it is analogical in the narrower sense in which, whilst the relations of terms are similar, the terms themselves are not enough alike to justify the inference. There are three types of ratiocination: (1) equations; (2) parallel cases of causation and of class-attribution; (3) analogies of imagination—on a level (as far as proof goes) with poetical and rhetorical ornaments, simile and metaphor. The natural progress of reason consists in advancing from the third type to the second and first, in which the minor premise becomes true; and this takes place because greater definiteness of thought has high biological value. Immature man, outside the necessary practical life (which might be called the biological life), is not a rational but an imaginative animal; and most savage beliefs about Magic and Animism are derived by analogies of imagination¹.

§ 8. THE WEAKNESS AND GRADUAL DECADENCE OF IMAGINATIVE BELIEFS.

There are, it is true, some facts connected with superstition which, with immature minds, may easily pass for good evidence. Some of those who pray to Neptune are saved from shipwreck, and the drowned

¹ A vague notion of causation plays a considerable part in some magical reasonings. Amongst other things, it seems to be assumed that opposite effects have opposite causes. In rain-rites the savage holds that

Who makes wet weather must himself be wet;
Who makes dry weather must himself be dry.

He sacrifices a black bull (or goat), the colour of clouds, for rain, and a white one for sunshine (Examples from Dr Frazer's *Spirits of the Corn and the Wild*, c. 5). The experiential grounds of these fancies lie in the opposite effects of rain and drought upon the crops, of noise and silence in hunting, of light and darkness upon all the ongoings of nature, etc.

Compare with this section "The Function of Relations in Thought," This *Journal*, 1911, iv., and Dr C. Mercier's *New Logic*, cc. xxii. and xxiii.

are forgotten. By suggestion the sick are often healed, and the hale struck down. Curses and incantations, if known to the intended victims, fulfil themselves. If a magician has the common sense to make rain only when the wet season approaches, the event is likely to confirm his reputation. Sleight of hand and the advantages of a dark *séance* are not unknown to a savage tutored in an old tradition of deceit. The constant practice by a whole village of both magic and industry for the same end, makes it impossible for ordinary mortals to see which of them is the real agent of success. For the failures of magic or sacrifice there are always notorious excuses. Hence the qualified expression 'almost entirely imaginative' has been used to describe those vast congeries of ideas which are characteristic of the immature mind as contrasted with the civilised: the foregoing analyses have shown their intrinsic structure.

Now, imagination-beliefs may seem indistinguishable in character from perception-beliefs; in immediate feeling-quality they are certainly very much like them; and, on a first consideration, they appear to have at least as much influence over men's actions: but this is not true. In course of time, they change, though the 'evidence' for them may remain the same. Moulded from the first by desire and anxiety, they remain plastic under the varying stress of those and other passions. In a primitive agricultural community, preparation of the soil, hoeing, reaping and harvesting go on (though with inferior tools and methods) just as they do with us; and from age to age the processes are generally (not always) confirmed or slowly improved. At the same time, every such process is surrounded by a sort of *aura* of rites, which seem to be carried out with equal, or greater, scrupulosity and conviction; yet, age by age, the rites slowly atrophy and lose their meaning and influence.

This weaker character of imagination-beliefs, their close alliance with play-beliefs, is shown in various ways:

(a) The rites which express them are often carried out with deception—practised on the crowd in a public performance, as by obtaining from heaven a shower of rice, which (over night) has been lodged in the tree tops, and is shaken down at the decisive moment; or, in private practice, played off on the patient, by bringing a stone in one's waist-belt and then extracting it from his body.

(b) Religious beliefs often combine incompatible elements, such as acknowledgment of the superior wisdom and power of a god, whilst employing devices to cheat or threats to punish him.

318 *The Conditions of Belief in Immature Minds*

(c) These imagination-beliefs break down under various trials:

- (i) Economy, as in selling the Rice-mother when the price of grain rises; or offering the gods forged paper-money, instead of good.
- (ii) Self-preservation, as in substituting the king's eldest son for himself.
- (iii) Compassion, as in burying with the dead puppets instead of slaves (though in this considerateness economy may have some part), or substituting in sacrifice a bull for a man.
- (iv) Social indolence and fatigue; whereby the meaning of rites is forgotten, and the rites themselves are gradually slurred and abbreviated. This must be an important condition of the degeneration of rites, as it is of language.
- (v) Foreign influence; even, perhaps, repeated experience of failure, etc.

(d) The beliefs of magic and animism are supported by intense emotional excitement during the performance of the rites and ceremonies that express them. Emotion is artificially stimulated, and probably is felt to be necessary to sustain the illusion.

(e) The specific connexion of such beliefs with the play-attitude of mind is shown:

- (i) By their rites being accompanied by games, such as leaping, swinging, spear-throwing—supposed to have some magical efficacy.
- (ii) The ceremonies themselves are often dances, dramas, choruses.
- (iii) With the degeneration of belief, the rites remain as dramatic and musical pastimes; whilst the myths survive in epic poems, fairy tales and ghost stories.

§ 9. THE UTILITY OF IMAGINATIVE BELIEFS.

These imaginative beliefs and practices were the necessary result of desires and anxieties about the necessities of life, at a time when men could not know any better, whilst a great development of free ratiocination was accompanying the growth of the brain from the anthropoid to the human scale. To us they seem so absurd, and the practices seem to waste so much time and care (at the best), whilst (at the worst) they are so cruel and destructive, that we wonder what utility can justify their prevalence and persistence.

There must be some great utility to compensate for the mischief, not in each case, but generally and on the whole; nor yet consciously aimed at, but accruing by the way. The utility of ethnic beliefs involved in the worship of gods, I have indicated elsewhere (*Natural and Social Morals*, c. ix. § 3), especially the political, artistic, and moral consequences; and I will now add some considerations as to the utility of those rites of magic and ceremonies of religion which more particularly subserve hunting and industry.

(a) They gratify the desire to do something, or to feel as if something were being done, toward the end desired, especially in the intervals when really effective work cannot be carried on, as whilst the crops are growing and after the harvest: they allay anxieties and give hope and confidence.

(b) So far as needs and interests are common to a tribe, village, or other group, these ceremonies encourage social cooperation and unity, and preserve tradition and the social integration of successive generations.

(c) But equally important it is that they are organized pastimes. The men of backward societies, during a considerable part of their time, have not enough to do. Social ceremonies keep people out of mischief, and, at the same time, in various ways, exercise and develop their faculties. With us industry is a sufficient occupation, or even too engrossing, and circumstances keep us steady; so that, in leisure, pastimes may be treated lightly. With the savage some pastimes must present themselves as necessary periodical religious duties, whose performance, in his belief, encourages and enhances industry. Our games are free from practical hopes and anxieties and are, for the most part, a merely personal recreation; but the more elaborate, such as horse-racing, have still a social function; or, like cricket and football, a tribal character. The school, college, county or even the nation feels deeply concerned about them.

§ 10. BLACK MAGIC AND RED RELIGION.

As for the dark side of magic and superstition, it needs no other explanation than crime, fanaticism and insanity. Love, jealousy, hatred, greed, ferocious pride and the lust of power, are amongst the causes that mould belief. Any calling pursued in secret, under a social ban, is of course demoralised. Where the interest of an organized profession stands in a certain degree of antagonism to the public

interest, it may become the starting point of unlimited abominations; and of this truth the interests of magicians and priests have furnished the most terrific examples. The retrospect of human culture fills you with dismay, but need not excite astonishment; for human nature is less adapted to its environment (chiefly social) than anything else in the world; the development of the mind and of society has been too recent for us reasonably to expect anything better.

§ 11. IMAGINATIVE BELIEFS AND SCIENTIFIC IDEAS.

If the character of those beliefs which distinguish the immature mind of savages from the mind of scientific culture has been rightly described in the foregoing pages, it becomes impossible to assent to the derivation of science and scientific ideas from magic and animism. Scientific ideas are rather to be considered as implicit in the practical life of animals and men in their dealings with nature, and as having been elicited especially from the practical pursuits of men by the 'dissociation of variable concomitants.' They are elicited from the experiences of hunting and war, but especially of industry and commerce; where the necessary succession of events of engrossing interest is felt in every action, and can also, for the most part, be followed by the eye, as in making and using weapons, tools, boats and in building houses; or where land and goods must be measured or counted and exchanges carefully compared. Causation and number are inherent in these operations; and when the man of genius arrives, in whose mind those relations have become isolated, he sees them in the facts of experience, and assimilates the facts under their appropriate relations. But superstitious beliefs, though (as we have seen) they obtain a certain empirical verification upon the assumption *post hoc, propter hoc*, are far from being verified constantly and continuously; nor is the process of their fulfilment, step by step, capable of being felt or perceived. And we have seen that the ratiocination by which such beliefs are arrived at in detail always takes place by a false or groundless illation.

As industry becomes more and more continuous and exact and arduous, it is more and more differentiated from play; and as it obtains control over natural forces, superstition declines. Uncontrolled forces are its stronghold; and the last prayer, like the first, is a prayer for rain.

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AN EXPERIMENTAL INVESTIGATION OF PERCEPTION.

BY FRANK SMITH.

(*From the Psychological Laboratory, University of Cambridge.*)

1. *Subjects of experiment.*
2. *Apparatus employed.*
3. *Method of investigation.*
4. *Results :*
 - (a) *Immediate interpretation of objects as a whole.*
 - (b) *Analysis of the objects perceived.*
 - A. *Experiments on Adults :*
 - (i) *Procedure adopted by the subjects.*
 - (ii) *Subjective factors.*
 - (a) *Imagery.*
 - (b) *Subjective additions.*
 - (c) *Objective changes in the pictures.*
 - (d) *Subjective certainty.*
 - (e) *Readiness of decision.*
 - (f) *Associations.*
 - (g) *Self-projection.*
 - (h) *Improbability.*
 - (i) *Influence of the first idea.*
 - (iii) *Perception of colour.*
 - (iv) *Feeling tone.*
 - B. *Experiments on Children :*
 - (i) *Six years of age.*
 - (ii) *Twelve years of age.*
 - (iii) *A mentally defective child.*
5. *General conclusions.*

WHAT are the factors involved in the process of perception? Are we justified in making a classification of individuals according to their 'type of perception,' as has been attempted by Binet¹? Do adults and children show any fundamental differences in their manner of perceiving? These are the questions which the following experiments were undertaken to answer.

¹ *L'Etude expérimentale de l'Intelligence*, Paris, 1903, ch. xi.

1. SUBJECTS OF EXPERIMENT.

The subjects in these experiments were thirty-two adults and thirty-eight school children of different ages. The majority of the adults were Cambridge graduates and undergraduates; with the exception of two domestic servants, all of them had had excellent education. There were seven women among the adults. Nine of the thirty-two were science students. The children belonged to three neighbouring schools—the — Grammar School, an elementary school in a good residential district, and an elementary school in one of the poorest parts of the town.

Experiments on the adults were carried out at the Cambridge Psychological Laboratory; in the case of the children I visited each of the schools in turn, and worked in one of the class rooms.

2. APPARATUS EMPLOYED.

In the Laboratory the apparatus I used was a form of tachistoscope devised by Mr Hales, which has already been described in this *Journal*¹. By means of this instrument transparent objects can be exposed for a very brief period, and where nothing is said to the contrary it may be assumed that the time of exposure in the present experiments was about one-thirteenth of a second. Variations of the time interval were tried occasionally, and will be mentioned in their place. In the school experiments I used a smaller and simpler apparatus, which had a photographic shutter in place of the pendulum. The time of exposure in this case was longer, and probably not quite so constant, the average time being about one-fifth of a second.

The effect of these very brief exposures was to spread out the process of perception (and apprehension) over a longer time than usual, so that the different factors involved therein could be noted with fair ease. Even subjects who were quite unaccustomed to introspection were able to indicate accurately what progress in the process they were making, and how the progress came about.

The objects were presented in the form of lantern slides. They varied in nature and in difficulty. They were chosen for various reasons,—simplicity, complexity, familiarity or strangeness. The pictures

¹ 1908, II. 244 ff.

numbered 1 to 7 were coloured; the remainder were in black and white. The following is a brief summary of their contents :

1. A railway track with an engine and passenger train travelling quickly towards the right hand bottom corner. There are two pairs of rails, and also a pipe running parallel to them. On the left are several posts and a green hedge. On the right the ground falls away.

2. The platform of a small station, with the rails also showing on the right. Several groups of people are standing about, the most prominent being an old lady and her son, who are going to part. Half of a bridge is visible, rising from the platform on the left and turning at right angles to cross the line. Behind the bridge is a mass of station buildings, and in the left foreground are rails and a lamp-post.

3. Two men: a sweep on the left with a bundle of brushes on his shoulder, and a gentleman on the right with frock coat and silk hat. A vertical line down the centre indicates that they are about to collide with each other in turning the corner.

4. The sequel to No. 3. The corner is now to the right and the sweep's brushes have struck the gentleman in the face and knocked his hat off. His glove is also falling.

5. A child's face with a circular background of blue. The child's hair is unkempt and his right eye is screwed up mischievously, thus drawing the mouth to one side. The upper part of a white garment is shown round the neck but nothing further can be seen.

6. A man is busy tarring a wall with a long brush, the tar being in a bucket on his left. Another man seen in profile on the right has just begun to expostulate with him and the former, raising his brush on the left and turning his head to the right, has tarred the face of a lady who is just coming round the corner of the wall on the left.

7. On a table at the bottom of the picture are a cat, an inkpot, a book and a vase of flowers. Above the cat, and quite near it, is a bird in a cage. At the top of the picture is a scroll containing the words: "A bird fancier."

8. A pug dog on the left and a Persian cat on the right—lying on a table, with their faces towards the observer.

9. A three-masted ship of the *Victory* type, lying broadside-on in calm water. The port holes are very prominent. On the right and beyond is another ship of the same type.

10. A lifeboat standing on the slips in a harbour. The slips are very prominent in the foreground. Beyond the bay is a town with several large buildings showing indistinctly.

11. A donkey, standing nearly in profile, with its head to the right. The ground is rough and rising to the right. The donkey's ears are stretched out, the hind feet together and the fore feet slightly apart.

12. A Brittany bed: consisting of a richly carved cupboard containing two beds, one above the other. The doors slide apart and are open. Kneeling on a ledge by the upper bed is a woman in the costume of a Brittany peasant, and there is someone sleeping in the lower bed. Outside the cabinet is a carved settee.

13. A muzzled bear in the middle of a suburban street, standing on its hind legs and holding a high pole. There are houses with prominent bow windows on

324 *An Experimental Investigation of Perception*

each side and the sun, evidently shining brightly on the right, is casting strong shadows.

14. An elephant standing in profile, head to the right. The trunk is curved upwards and near it is the keeper. There are trees in the background and some people show faintly on the right.

15. Six yachts, at varying distances, sailing up a lake which is backed by ranges of hills. All the yachts are inclined towards the right and there are ripples on the water.

16. A chaos, made by a few blots of ink. To the left are a few straight lines which suggest a box or a small hut, but the rest is without meaning.

17. Queens' College, Cambridge, as seen from Silver Street bridge. The college buildings are on the right, and dense trees on the left. The river flows between, and is spanned by a wooden bridge near which are two small punts.

It may be here remarked that it was generally impossible to foretell whether any given picture would prove easy or difficult to a given subject,—some subjects finding pictures very difficult which caused no trouble to others, while the latter perhaps stumbled at those pictures which appeared easy to the majority.

3. METHOD OF INVESTIGATION.

The experiments were carried out in the following way: first, the subject was told something about the nature of the experiment, and the part he was to play therein. He was then shown exactly where the image would fall on the screen, and was also habituated to the speed of exposure. If desired, a small fixation mark was affixed to the screen. A lantern slide was then put into place, a warning signal was given, and about a second later the exposure was made. The subject reported as fully as possible what he had seen; he was asked also to describe any associations the picture had called up, any feelings, emotions, etc. he had experienced, any inferences he had made, and so on. This introspective record I wrote down almost *verbatim*. After the subject had told me all he could remember, I questioned him in order to clear up any vagueness of statement and to make sure that nothing had been forgotten. When the report on the first flash was completed, a second exposure was made of the same picture, and the subject reported the additions or corrections he wished to make. This procedure was repeated several times, until the subject had seen the whole picture, or had become tired of it, or had found it too difficult to solve completely. Hence the number of exposures varied with each picture and each subject, sometimes five or six being found sufficient, and in some cases a hundred not being found too many. Occasionally, after the warning

had been given, the picture was not shown but the subject was asked to report his experiences while he was preparing to attend to the coming flash. The records of these 'fore-periods' threw light on the subjects' activity in planning out their work, and showed how far they attacked the problem systematically.

4. RESULTS.

An analysis of the introspective records leads to the following results:

The process of perception, as it here occurs, takes place in two stages. There is first (*a*) an immediate interpretation of the object as a whole, and next (*b*) an analysis of this vaguely apprehended whole into its component parts. Subjects of all ages showed striking uniformity in the first process, whereas in the second there was a no less striking variability.

(*a*) The interpretation is immediate, and my experiments did not throw much light on this point, though with other material it might be possible to investigate it more completely. "We begin," as Stout says, "by apprehending a whole in its distinctionless totality, and then proceed to unfold its details¹." It is important to insist that previous experiences, which give meaning and distinction to this complexity, need not themselves be present to the mind. Some writers have insisted very strongly on the necessity of the revival of past experiences in perception, but these experiments support the view that the mind can give a meaning to experience without any such revival taking place². For the time being we may go no further than this general interpretation. "It is possible to think of a whole in its unity and distinctness, without discerning all or even any of its component details³."

As examples of this instantaneous recognition I quote from the introspection of different subjects:

"Recognition is quite instantaneous. I didn't notice any *process*."

"I couldn't distinguish between the instant of seeing and recognition."

"Recognition was immediate."

"Recognition is very rapid, and the word is as quick as the recognition."

¹ G. F. Stout, *Groundwork of Psychology*, 1911, 70, 71.

² Cf. "What is Perception?" by C. H. Judd, *J. of Philos., Psychol., and Sci. Methods*, 1909, vi.

³ G. F. Stout, *Analytic Psychology*, I. 78.

326 *An Experimental Investigation of Perception*

In rarer cases recognition, although immediate, developed greater clarity after a perceptible interval. Thus:

"Recognition is quite immediate, but it gets much clearer as one thinks about it."

"Recognition is instantaneous, though there's something like an unfolding."

A few subjects (at least three of the thirty-two,—probably more, as I did not question all about the matter), distinguished between the recognition of differences in luminosity and the recognition of form. That is, they first saw an illuminated patch with certain black marks, and then these immediately assumed some definite shape. Examples of this distinction are found in the following:

"A white background and black things in front. It looked like acrobats in all sorts of attitudes. There was a slight searching for an explanation, but not long." (No. 16.)

"My first impression was of black masses without outline. Then it suggested a dog."

I am not sure that these cases are as genuine as they look. Two of the three subjects were quite untrained in introspection, and the third gave very few answers of this nature. In one of the subjects I think it was due to excessive caution. This subject always began with the most general interpretation he could, and only approached the particular after great deliberation. Thus in picture No. 6 he described the central man as follows: "I saw something like an inverted Y. I take it to be a man." And in picture No. 11 he said: "The picture of a quadruped. There is something sticking out of the head—horns or ears. I associated it with a cow."

It will be seen that by this method of answering he left room for retreat in case of mistake, and gave first an interpretation broad enough to avoid nearly every likelihood of error. In one of the remaining two subjects I think more was seen than she reported at first—either through vagueness or forgetfulness. In the picture of the bear (No. 13), she said:

"Nothing but white and black. I couldn't say what, unless the white is snow. I was quite ready, but not so concentrated as usual. It might have been a boy standing up in a snowy street. But I dismissed that as impossible. It was all too blurred. He came up involuntarily."

I then asked whether any more came back to her, even vaguely, and she replied: "Well, the boy might be standing with his legs apart." It is therefore probable that the perception of form here was more complete than the first report indicated.

The third case of this kind was one who had had long training in psychology and in experimental work. With the nonsense picture (No. 16) she described her experience as follows :

"I can't say much of the first stage. The picture rolls on, but doesn't take a final interpretation at first. I feel there is a preparation for a definite final something. I had confidence of something definite which would come up when the picture was gone. Then I saw other parts, and the previous interpretation was choked down by this new sensation, and so the other never appeared in full consciousness."

Perhaps I have unduly emphasized these few exceptions, but it is because they are the more striking in face of the uniformity found with the remaining subjects. In my own case I had the experience of a blinding flash of light from which the awareness of certain forms immediately emerged. That is, while I was still in a state of surprise at the speed and brightness of the exposure, I found I was conscious of what I had seen, without the least effort being necessary. The whole process was one and indivisible.

In order to try and discover something more about this point I modified the conditions of the experiments by cutting down the time of the exposure to considerably less than one-hundredth of a second. Yet even with this very short exposure the immediateness of recognition was unchanged. I quote the following reports :

"Recognition was as immediate as before."

"A three-masted ship of the type *Victory*. There are port holes and white band. It is facing my right. The exposure was considerably shorter. Open sea. No land in the background. I have an impression of small boats, but I am uncertain. My certainty about the general thing is just the same, but the details are more uncertain. The recognition of the three-masted ship was just as immediate as it would be with a longer exposure." (No. 9.)

Subjects were usually unconscious of any change in the speed of the flash in the cases where I secretly altered it from the normal to the very rapid time. They often reported, however, that the picture had become "very dull," and conversely, when the duration of exposure was lengthened, they would remark that the picture had become "much brighter."

I also tried the effect of showing a picture which was outside the experience of the subjects. This proved to be a difficult matter, and pictures Nos. 12 and 16 were the best I was able to obtain. Both were interpreted very differently by different subjects, but it is significant that there was never any hesitation about interpreting them as something of every-day experience. The former of these pictures received such descriptions as the following :

328 *An Experimental Investigation of Perception*

"It looked like the balcony of a window and somebody standing on it."

"Something like the frontage of the Laboratory, but much more pretentious. I was not aware of any interval between the sensation and the interpretation."

"It was like a glass case, standing in a sort of trough. Immediate interpretation."

In the case of the latter picture similar fantastic accounts were given:

"It looks like a most extraordinary coat of arms, made up of quite unintelligible figures."

"Like a cartoon in *Punch*."

"It might be a lot of rapiers and boxing gloves—the paraphernalia of a gymnast."

"Three little animals with funny little heads."

Although interpretation was so immediate, doubt might afterwards arise as to its accuracy, but this never prevented an immediate judgment being made in subsequent experiments. Even the faintest sensations will bring about the perception of complex things. Picture No. 12, by its regular form, might suggest a door or a window, or the part like a settee near the floor might even suggest a fireplace and fender. Hence when the picture was incompletely and imperfectly seen, ideas connected with one or other of these interpretations at once arose, and the picture was filled in mentally by association and expectation. In this way so strange an object was interpreted in terms of very familiar objects.

Our experience in normal life is often very similar. No doubt, we repeatedly misinterpret the things about us in our rapid glances to and fro, and we only look more closely when doubt arises, or when we are greatly surprised at what we have seen. I have occasionally made most grotesque mistakes in glancing quickly along a crowded street while thinking of something quite remote from the scene; a second careful look has failed to reveal any very obvious reason for the first interpretation.

It is interesting also to note that many young children, who had at first seen No. 12 as a window or a door, persisted in the same interpretation when they saw the picture properly at the end of the experiment. Here the mental factors at work were apparently much stronger than the visual sensations which were responsible for the process.

(b) After the immediate recognition, which, as I have shown, is of universal occurrence, the mind seeks to know the details, and to analyse the vague whole into its various parts. In this process of analysis, enormous individual differences appear; indeed, at first sight, it is difficult to discover any uniformity whatever. I have found it vain to attempt to set up 'types' of individuals with distinguishing and

constant characteristics, because the peculiarities of what I thought might be a 'type' rarely turned out to be constant, and often appeared in other subjects whom I had decided on other grounds to belong to another type¹.

A. *Experiments on Adults.*

I propose to give, first, an account of the introspection from adult subjects, reserving the children's introspection till later, in order that a clearer comparison may be made between the two groups.

(i) *Procedure adopted by the subjects.* Subjects may be roughly divided into two classes according to their method of procedure during the analysis. Some continued to see each picture as a whole, except when an outstanding feature attracted their attention or happened to fall on the focus of vision. Sometimes one detail would appear so prominent that it was seen continually, even when there was a faint intention to look away from it. Other subjects made a more or less systematic search for details. In the most pronounced cases of this class, they were able to concentrate on an exceedingly small area of the screen, and so saw only a very minute portion of the picture clearly and the rest very vaguely:

"The clear part was a small semi-circle at the bottom."

"One seems to see just one part at a time."

The subjects in the first class made, in general, very little progress in the solving of the pictures, and, indeed, often asserted they were making no progress at all. Typical reports are:

"I haven't got much more than at first."

"It seems to me one gets more out of it the first time. I suppose that my mind gets confused."

"I think the second time was as clear as any time since."

These subjects did not usually persevere much. They began to feel baffled after a few exposures, and unable to proceed:

"I am gradually getting the feeling I am stuck and can go no further."

"I am beginning to feel hopeless about it. With increased hopelessness comes increased impatience to see the picture in a long exposure."

¹ "Moreover it happened that any subject whatsoever, who from his description of one picture must have been classified as an 'observation type,' would behave in the description of the next picture as if he were of the 'description type.' We have therefore preferred to abandon this attempt (to classify by types), rather than undertake an arbitrary and artificial classification." Marie Borst, "Untersuchungen über die Erziebarkeit und die Treue der Aussage," *Beitr. z. Psychol. d. Aussage*, 2^{te} Folge, 1905-6, 105-6.

330 *An Experimental Investigation of Perception*

Those, on the other hand, who searched closely for details made steady progress and were content to spend a long time with each picture. They showed very great anxiety over their accurate procedure :

"I was too high. I don't get it unless I hit the exact spot."

"I seem to see only the point where my eyes are concentrated."

"After the first impression I should naturally go for the details."

This power of minute observation was most strikingly developed in students of science, and the two best cases were those of a University Lecturer who has spent many years in scientific investigation, and a Research Student of some years' experience. I will quote from the report of the latter, given with picture No. 3, and in order to show the development of his analytical method I will present his answers after the first flash and after the tenth flash. It will be seen that all parts of the picture are mentioned in the first, but only a very small area in the second answer :

"A sweep and a gentleman. The sweep is coming round a corner apparently. The well-dressed man has a top hat and a cut-away coat. He can't see the sweep apparently. It may not be a corner, but there is a line between them which suggests one. The sweep is black, and has brushes over the left shoulder. I believe he had something in his left hand. It looked like a bag without handles, as though for soot. The upper part of the picture was more definite on the whole. The gentleman had a moustache ; I am not so certain about a beard, but I think he had one. There were no bricks marked in the wall. I think it was a corner, because each one seemed ignorant of the other's approach. There is something on the right of the sweep in the distance which gave me the idea of a lamp. The gentleman was a tiny bit bent. The sweep had the attitude of a man carrying something. He looked slovenly. I couldn't see the top of the wall : it went straight up out of the picture. I didn't see the edge of the pavement if there is one."

I asked him if the picture was coloured or plain, and he replied :

"Plain. It was not a photo. It may have been done with ink and brush. I didn't see the picture in detail enough for that. It reminded me of a picture I saw two years ago in Birmingham."

I further asked him if he saw all these things at the moment of exposure, and he answered :

"I keep recalling the image, and I can recall almost as faithfully as I saw. The association with the Birmingham picture came, however, at the moment of exposure, but got dropped for a time."

This long description, it will be seen, gives a comparatively accurate and full grasp of the picture as a whole, with a very probable explanation

of the relation between the two main figures. But at the tenth flash his attention was turned to one part only:

"I saw the sweep's face and eyes. They are not very dirty, but a little black. They are in profile. He seems to be in a hurry judging from his face. His eyes are fairly wide open. He is too boyish for a beard. The nose is fairly prominent. I am not quite certain whether there is a moustache or not. It may be the shadow of his nose. I did not see his ear. I haven't seen his boots at all. And I'm not certain about the pavement. I'll look at the right bottom corner—then I shall want to go to the other corner and compare."

When we compare the following answers from other subjects with those just quoted we see clearly the difference between the two classes:

"A face; I think a woman's." (No. 5.)

"A street." (No. 17.)

"Two men fighting. Then I thought they might not be fighting, but clasped in one another's arms. The one on the right had light trousers on." (No. 4.)

These are the complete reports after the first exposition of a picture.

Subjects of the second class, who analysed fairly thoroughly, usually reported that the pictures gradually lost their unity and became mere collections of parts:

"It is made much more now of pieces put together than at the beginning. This is due to seeing different parts separately."

"It is made up of parts and distinctly less unified."

"The picture seems to have got divided into two parts." (No. 3.)

"The more I see the picture the more I take bits of it. If you showed it me twenty times I should never take it all in."

"It needs an effort to recall the whole."

The same effect was produced when I showed pictures to some subjects with an exposure of two or three seconds: "I got no impression of the whole." Once, when I changed the time of exposure from one second to one-thirteenth of a second, a subject remarked: "The picture was more of a whole then, without time to concentrate."

There was one case, and only one, where mention was made of the reverse effect of seeing the picture as a unity more and more as the experiment proceeded: "The picture is now a whole, and I see everything vanishing away to a point about the middle of the picture. There is more unity than at the beginning."

The group with more strongly marked analytical powers showed more systematic procedure and mental activity¹ than the others in

¹ Cf. A. J. Schulz: "We might speak of an 'active' and 'passive' type. The first analyses the exposed object—compares, unites, separates. The other behaves more

332 *An Experimental Investigation of Perception*

many ways. They constantly planned out their procedure beforehand and fulfilled their aim in spite of all obstacles :

"I was going to look on my right. I had a sort of mental struggle first and then decided to look for the people on my right."

"My attention to the man was intentional." (No. 14.)

"I'll look to the stern." (No. 9.)

"I can't make out the people on my left. I must give attention there." (No. 2.)

This systematic procedure is well illustrated by the following extract from the introspection of the University Lecturer already mentioned. The picture is No. 1, and the figure before each answer refers to the number of the exposure :

(35) "Let me think what to do. I think they are ordinary sleepers. I'll look."

(36) "I got a general impression then of green lines—actually on the lines. It doesn't seem quite normal."

(37) "Yes, the same. I was a bit too low."

(38) "I couldn't get the nature of the sleepers. There's a tremendous lot of green which seems out of place."

(39) "I am not sure if there aren't more than two sets of lines. I can't be certain about the number."

(40) "My impression then was of four lines and a rail to one side. I had that before. It may be a pure illusion."

(41) "Yes, an impression again of more than two sets of rails. I can't be certain about the number."

(42) "I am sure there are more than two pairs."

(43) "I think the train is on the far rails. But I think there is more than one pair there. It is frightfully difficult to decide."

(44) "I can't be sure whether I am taking this rail on the left from its mate. I have an idea of more than four. I'll give it up for a bit."

This is an excellent instance of consistent application to the solution of a small point; it is but one of numerous similar instances given by this subject throughout the experiments.

Some subjects were only guided to an active search by some kind of outside stimulus. The stimulus might come from a part of the picture which had aroused curiosity, or arise from the monotony of seeing the same thing several times :

"I feel now I want to look at the expression, to find what it is."

"I am bored with the train, and am looking round it."

passively, and lets the picture simply 'work on him,' in order to enumerate the elements afterwards one after the other, without concerning himself about their relations. However, a sharp distinction is impossible. Under favourable conditions, and for certain relations (especially identity or strong similarity) all subjects were active—of course in different degrees. But the same subjects were not constant." "*Untersuchungen über die Wirkung gleicher Reize auf die Auffassung bei momentaner Exposition*," *Ztsch. f. Psychol.* LII. 251.

In total contrast to this active direction of the perception process, some subjects showed a listlessness and lack of method which is best called 'passivity.' Sometimes it arose merely from lack of interest, as in the following:

"I feel more passive and less curious about details."

"I have been letting things come as they will."

"I haven't tried to find any explanation."

The difficulty of a picture was sometimes enough to prevent active effort:

"It's an awful effort to look at one thing. It's easier to be passive than active."

"It is more difficult to concentrate on a detail in this picture—there's such a lot to see." (No. 2.)

Certain outstanding features of a picture would often attract attention, and would prove stronger than the subject's intention to look at another part:

"The curve of the trunk attracted me again." (No. 14.)

"I meant to look that time at the side, but the engine again attracted me." (No. 1.)

"I meant to look at the two people, but the other platform caught my eye." (No. 2.)

"I can't get away from the bear in the foreground." (No. 13.)

Indeed sometimes this attractiveness of striking parts prevailed over the opposing intentions of subjects who showed systematic power to direct the process:

"The rails always catch my eye. I didn't want to see them then." (No. 1.)

"The forepart is so conspicuous that it attracts my attention."

I agree with Schulz¹ that most subjects show the characteristics of either 'type' at different times. But among my subjects a few could clearly be designated as 'active' (nearly all of these were science students); while at the other extreme some subjects were almost wholly passive (science students were not found in this class). There remained, however, a large group, the members of which showed at various times certain characteristics of both classes (among these were a few of the science students). The majority of this group were often active in the fore-period: they would, for example, decide to look for some definite detail, or to settle some doubt; but in the actual exposure their attention would be passively diverted to some outstanding feature, and their intention would not be carried out.

¹ See footnote, pp. 331, 332.

334 *An Experimental Investigation of Perception*

The advantages of a scientific training were well-marked: a rough calculation shows that the science students made three times as many inferences as the others¹.

Several of the almost wholly passive subjects made no inferences whatever, not even, for example, the obvious inference that when there was a shadow marked in a picture the sun must have been shining. This was the commonest inference of all, and was made occasionally by children. No doubt all subjects could make this inference if directly asked; the important point is that they did not do so spontaneously. One subject noticed that the feet of the central man in picture No. 6 were turned away from him, yet he only remarked on it as a curious fact: he certainly did not infer that the man's back must also be turned towards him.

Active subjects not only made more inferences than passive ones, they also carried them further. Thus, they not only saw that the sun must be shining in picture No. 13, they also added that it must be rather high in the sky, because of the fairly short shadows. Two subjects decided that as there were three or four posts on the left in picture No. 1, therefore they must be telegraph posts and not signals; whereas most subjects were content to leave the question open. Other typical inferences that were made were the following:

"The wind must be blowing from the left." (No. 15.)

"The train must have been photographed from a low position." (No. 1.)

"It must be a street corner, because the sweep and the gentleman do not see each other." (No. 3.)

"The bear's face must be towards the stick, because it is all in shadow."

Inferences were not always so correct as these I have quoted, and sometimes led the subject quite astray. Thus in No. 14, one subject thought he saw fur on the animal's legs, and inferred a shire horse, and this idea persisted for a very long time. The ears of the donkey were occasionally seen as horns, a cow being inferred. The bear was often seen as a man in the first flash, holding a brush, and the picture was inferred to represent a crossing-sweeper. This interpretation occurred in a surprisingly large number of cases, and occasionally could only be dismissed with difficulty.

Hence a wrong inference at the outset might add very considerably to a subject's difficulties, though, as a rule, one who could infer quickly and at the same time had independence of judgment and activity of

¹ These facts, while they do not prove that a scientific training will produce an analytical power of observation, suggest a high degree of correlation between the two.

outlook, had a strong advantage over others. By inference he was able to arrive at various possibilities and hypotheses to which he might turn his attention.

So far as I could tell, inference was weaker in women than in men. Of the five women subjects whom I investigated fully, inference was practically absent in four. By the remaining subject only very simple deductions were made, such as the position of the camera which took the picture and the length of the exposure (this subject was interested in photography). The limited number of cases, however, makes it impossible to speak on this matter with much certainty.

Pictures were divided by the subjects into two classes: simple and complex. The simple were those whose subject-matter was only one thing—a donkey, a bear, etc. The complex were those which related an incident, and whose meaning was more or less hidden, and depended on the discovery of the relations of the figures to one another (Nos. 2, 3, 4, 6). Now detail obviously plays a smaller part in the complex picture than it does in the simple, and subjects who could make an accurate and exhaustive survey of a simple picture did not always arrive at a satisfactory solution of the incident pictures. Hence it was that some subjects excelled in the incident pictures without any very careful analysis of details. This was well expressed by one subject in passing from No. 1 to No. 6:

"I feel here a marked difference of perception. In the engine picture it was a question of details. Here it is a question of the sense of the story, for which details are unnecessary. This is a proof in the case of art that pictures that tell stories almost necessarily force to superficial contemplation. I have a feeling that I have seen less of this than of the train, for the reason that I was occupied with the story it tells."

Another subject said:

"The parts hardly exist in a story picture. Details would be a nuisance. So I keep to the story interest."

One subject, however, said that details were far more important in a problem picture "as the problem is greater: the figures might be doing a thousand things." But this subject was not strikingly successful in solving such pictures.

The differences between scientific and other students can be expressed more accurately by figures. Thus, if the number of objects and qualities of objects correctly mentioned by a subject in a given number of pictures be tabulated, we shall be better able to see how far a division into classes is justified. It is significant that of the five subjects who stand

highest in such a table (with an average of 30.6 marks for each picture), four had had scientific training, whereas of the five lowest on the list (with an average of 13.3), only one was a scientist, and he was the highest of those five. The remaining three science students occupy the sixth, ninth and twentieth places respectively on the list. This shows that science students possess a decided advantage, in general, over other students as regards the discovery of the contents of an object, but that this advantage is not invariably possessed by all such students.

We may carry out this method of tabulation still further. In giving correct reasons for things, four of the five best subjects were scientists, but only one of the five worst. Similarly, of the five who mentioned the largest number of correct positions occupied by different parts of the pictures, three were scientists, and of the five worst in this respect only two were scientists. In describing the movements or actions of the figures in the pictures, four of the five best subjects and only one of the five worst had had scientific training.

Of the five who made the fewest mistakes and the five who made the most mistakes in each picture there was only one person of scientific training in each group. But to get a correct measure of accuracy we must divide the total number of mistakes by the number of objects correctly seen, since subjects who mentioned a large number of objects ran a far greater risk of error than did those who mentioned only a few. Of the most accurate five determined in this way three were scientists, whilst among the least accurate five there were no scientists.

In every respect, therefore, the scientists taken as a group proved superior to the others, but this superiority did not necessarily apply to them individually.

(ii) *Subjective Factors.* There are a number of subjective factors which can be best treated separately before an attempt is made to deduce from them more general conclusions.

(a) *Imagery.* In varying degree visual imagery was, of course, very common among the subjects. It was absent in two cases only. In one of the cases it was replaced by "a sort of verbal memory." In the other case there was a nearly complete lack of all kinds of imagery, and this subject frequently complained of his disadvantage. Thus, he found great difficulty in locating special parts of the pictures in the fore-periods. His method was to examine very small areas at a time, and he tried to fixate the exact spot before the exposure, but as he had no image he frequently made mistakes of localisation. He was also more liable than most subjects to forget to tell me all he had seen unless he told me at once.

As a rule visual imagery was a very useful aid. There were cases where it seemed to be almost as clear as the percept, and could be resorted to, even after a considerable interval, in order to settle some doubt or answer some question, much as one would look, under normal conditions, at the actual object. I quote some examples:

"Just after I said that I wondered where his feet would be. I argued that his right foot would be behind, so I recalled the image, and I think it is so."

"One moment! There are railings, I remember now. I was thinking, and saw them in my image." (No. 14.)

"I believe I project the image on the glass and compare the two."

"I can recall the image almost as faithfully as I saw it."

The subject from whom I have last quoted thought he saw the sweep coloured blue in one exposure, though he was not able to see the blue again in later exposures. Yet he could revive either of the two images—with and without colour—quite easily, and he realised that he had only seen the blue sweep once. The two images seemed to remain quite separate and constant in his mind.

In many cases imagery was selective, and might be good for form and not for colour, or good for outlines and poor for detail, and so on.

A most interesting discovery was the peculiar behaviour shown by the visual image in a few subjects, in that it seemed to possess a 'self-activity,' and began to change and develop almost at once. That a mental image changes as time goes on is well known¹, but the change in this case seemed to begin immediately. There were five clear cases of this kind, and one was specially remarkable. The pictures were intensely real to this subject, and her image had all the movement and change that reality itself has. I quote first the most important parts of her report on picture No. 13 (the numbers again indicating the different flashes):

(1) "There was a street and it was sunny. Whether there was a man or a bear with a pole or not—it's something with a pole. It's something too big for a man, and looked like a bear standing on its hind legs. It felt hot and sunny."

(2) "I feel sure it's a bear with a pole in its paws. It is still hot and dusty and sunny."

(3) "I am quite sure it's a bear. It seemed brighter and sunnier. I had rather a feeling of fear—I didn't like to see a bear in a thoroughfare. There was nothing round about it."

(4) "I see the shadow of the bear distinctly on the left. The background seems fearfully indistinct. I can't get away from the bear in the foreground. Some

¹ "The imagery tends, with the lapse of time, toward the imagery of the object represented by the picture, and with this change takes on characteristics that belong to the object, but which are not represented by the picture." F. Kuhlmann, "On the Analysis of the Memory Consciousness," *Amer. J. of Psychol.* 1907, 411.

338 *An Experimental Investigation of Perception*

movement seems to go on in my visual image on the left. There is a man in a straw hat, and people crossing the road a long way behind the bear. I still feel it's a fear-some thing."

(6) "Yes, there are houses on the left and something on the right. Still dusty and sunny and a foreign land. I don't like seeing a bear on the road."

(8) "...I have a feeling of wishing they would take the bear away."

(9) "...I don't know how much I see and how much I imagine, because I feel it is in a big town, and the bear has no business there."

(12) "I feel there are two men about, but I don't see them. There's still that man in a straw hat walking across the road, though he's only in my image."

(16) "I see the men in the after-image—the keepers. I don't see them in the picture. There's a lot of movement afterward in my image."

In this case the cause for these additions seems to be her own fear because of the unusual sight of an unattended bear in the streets. It will be noticed that she is *conscious* of the 'activity' of her image. I quote a second example from the same subject where the additions seem to be caused more by associations than by imagination. The picture is No. 1:

(1) "A Great Northern train coming towards me. I can hear it and I feel that it is going towards a tunnel. It is certainly Great Northern and going to Scotland. I imagine I saw a child waving a handkerchief on the left. I still hear the noise of it—puffing and going. I imagine luggage and people and everything. There's a tremendous lot of movement."

(2) "...It is full of people in all the carriages. I have a pleasant feeling of excitement about it. I'm sure it's on a long journey."

(3) "...I still feel somebody is waving out of the window to my left but I can't see them. I still feel it is going, and hear and smell it."

(4) "...I was looking for the person on my left. I couldn't see him, but I feel he's there...I see the people in the compartment—the way they are dressed and everything."

(6) "...I still feel it's about to go through a tunnel. I feel the landscape changing as the train goes on. The people all look like travellers—as though they have settled down for a long journey. It couldn't be a local train."

(7) "I think some of the windows are open. But I don't see them very distinctly. It's slowing down a little. I don't know why."

When I showed the picture at the end she added:

"I was going to Edinburgh by day by the Great Northern—though I have never been to Edinburgh by day. Yet I felt it was the same journey—though I was sure I was not going to London in this case as I was when I travelled on that route by day. The people were a mixed crowd—Americans and such like. It was not a crowd on a local train. There was a woman with a veil. I have to pull myself together to see it as a picture—it's so absolutely real."

This case was rather exceptional, though there were four others who belonged to the same category. One of them, in looking at the donkey,

got an idea that there was also a pony's head somewhere attached to the donkey, and this idea persisted with varying strength through several flashes, though he added:

"I am not sure whether I see this or whether it is imagination. It suggested that to me. I saw an image of a pony nibbling. It is a matter of perception at first but, may be, worked upon in my image."

Evidently he was not sure exactly what had been perceived and what had been imagined.

Subjects with mobile imagery may easily be unconsciously untruthful¹, and mistake imagination for fact. Such people are known in ordinary life, wholly unconscious of their failing. But, as I have shown, a subject with mobile imagery may be fully conscious of the difference between image and percept.

Other kinds of imagery than visual were not very common. Olfactory and auditory images were only mentioned by one subject, verbal-motor imagery by three. Unpractised subjects probably overlooked the nature of their imagery in many cases.

(b) *Subjective additions.* Very closely connected with the phenomenon of self-active imagery was the tendency shown by some subjects to make additions from their associations and imagination. This tendency was shown by all the five subjects who had self-active imagery, and also by a few others in a less marked degree, one of whom possessed either no visual imagery at all, or so little that it was of no importance in his mental life. However, it was scarcely more than a tendency in his case, about which he was cautious it is true, but which he could keep in control. Thus, he said: "I am almost ready to persuade myself I can see a man on the smallest provocation."

Other typical reports dealing with subjective additions were the following:

"I feel now I could see anything I wanted in this face, as regards expression."

"I thought I saw a turban on the figure." (No. 14.)

"I think there are some trappings on the elephant—part of a howdah."

"My mind wanders afterwards, and seems to raise up new things."

"I thought bulldogs wouldn't have ribbons on, and so I seemed to see heavy collars."

¹ See "Sur les transformations de nos images mentales," by Jean Philippe, *Rev. philos.* XLIII, 482: "When a new state of an image has replaced the former state of this same image, it presents itself alone in the place and instead of the former one: we cannot therefore compare these two successive states of the image, nor see, consequently, that it has changed—unless a fortuitous meeting reveals the transformation that has occurred."

340 *An Experimental Investigation of Perception*

It would be an interesting question for future inquiry whether self-activity of imagery is a necessary cause of subjective additions. In some cases, of course, they are the same thing. Then it is the image which is described, not the object.

(c) *Objective changes in the pictures.* Ten of the subjects reported that there were inconsistencies in the successive percepts, as though the picture itself changed during the course of the experiment. This phenomenon occurred, of course, mostly in people of strong subjective tendencies. The cause probably was that they would see a part of the picture, interpret it immediately, and complete the unseen parts mentally. In a later flash they would see a different part and find certain discrepancies. Their experience was thus like seeing two different pictures. Typical reports were:

"I am not quite sure it is the same picture."

"My notions are continually clashing."

"The train always appears more in the foreground of the picture than I expect it to be."

"It seemed a different picture of another donkey."

It is interesting to note that the two scientist subjects who showed such great similarity in their method of procedure belonged to different classes in this respect: one spoke of objective changes several times in each experiment, the other never mentioned them.

In a few cases the pictures seemed strikingly larger or smaller at the end of the experiment than they appeared in the flashes. With one subject each was definitely smaller at the end. With different subjects this change occurred most often in the case of the train picture, and many were surprised to find how small the engine really was.

(d) *Subjective certainty.* Subjects varied considerably in the certainty and confidence they had of their decisions and achievements. It must be remembered that subjective certainty is no guarantee of accuracy; it is merely a mental attitude which may accompany sufficient or wholly insufficient reasons, and may be found accompanying utter inaccuracy.

Thus, one subject, who was very certain throughout, mistook the bear for a man. I allowed the mistake to go on for some time before I told her she had made a mistake—that it was not a man at all. She was quite unshaken in her opinion: "But it's a human being. If it's a woman she's in a man's get-up. You are fooling me." I added that it wasn't even a human being, and she replied: "That upsets all my ideas then. You haven't shaken my faith though." This certainty

persisted in subsequent flashes: "It still looks like a human being."
"Still the same exactly."

The certainty of some subjects, on the other hand, was obtained by reasoned argument, and by deduction from observed phenomena: "I saw three posts. They must be telegraph posts then, and not signals."

Some subjects showed a complete degree of uncertainty, which sometimes persisted through the whole experiment. Those who were uncertain accepted corrections readily, whereas the certain often refused them, or only accepted them after a struggle. One who had seen the bear as a man for twenty-one flashes was then told that it was not a man. After the next flash he said:

"Well, then, it's a bear dancing. The position of the legs gave me that, and it explains the shortness of the legs which I attributed to an apron."

This attitude of certainty or uncertainty did not seem to run parallel with any other characteristic, though on the whole the certain subjects made far better progress than the uncertain ones.

(e) *Readiness of decision.* Some subjects decided easily and at once, however insufficiently they had seen the picture; others remained undecided, divided between alternatives for a long time¹. The former were, as a rule, the least careful in their examination of the object, and wrong decisions did not affect their procedure in subsequent experiments. Children belong almost uniformly to this class, as we shall see. Many adults were quite uncritical as regards their own opinions. And when such subjects did change their opinions they decided on a second interpretation quite as readily as they had previously done on the first.

Sometimes a subject would begin his report in an undecided attitude, but would achieve a final decision by the end of it:

"It might be a railway station. The left part is suspiciously like a station. I saw people. Yes, I believe it's a railway station. Yes, it's a railway station." (No. 2.)

The subjects who only arrived at their decisions after an interval of some duration were generally very painstaking in their attempts to see everything of importance. They carefully weighed alternatives and examined possibilities. I quote from a case where, after deciding that the

¹ "With a few subjects the answer once given must remain unchanged. Whether it was right or wrong, whether a complicated figure was nearly round or angular—nothing or exceedingly little could be said. Other subjects, on the contrary, were able after a still longer time to criticise and correct the given answer, to find excrescences (*Ausgelassenes*), and so on." Schulz, *op. cit.* 287-8.

342 *An Experimental Investigation of Perception*

bear was a man in the first flash, the subject began to suspect a mistake after the eighth flash. His subsequent reports were :

(9) "I am beginning to suspect it isn't a man at all—it's a performing bear. It's the way bears hold a staff. I was looking then to see about this."

(10) "I saw his legs. They seem to be short and thick for the body. He's not a typically built man if he is one. The legs are a considerable distance apart."

(11) "The whole suggestion is that of a bear. I didn't see any ears, but the head and especially the shape of the figure suggest this."

(12) "I seemed to see the face, which is in profile. It is elongated and more like an animal's."

(13)^a "I saw the other leg. It seems to be short and without calf."

(14) "I seem to see that the back of the head is hairy. But I'm diffident about details of that kind. It's either an animal or a man dressed as an animal. From the figure I should say an animal, and probably a bear."

In extreme cases there was complete indecision. This occurred both with ignorance of the subject matter, and also with complete knowledge of it. I quote briefly from one case—the actual introspective record is very long—where the subject was very well acquainted with animals. The picture was that of the donkey (No. 11):

(1) "A donkey."

(2) "More like a cow. Hind legs are those of a cow at any rate. Head indistinct."

(5) "I can't associate the neck with any beast."

(6) "It seems to be a cow's legs."

(7) "I should still say a cow, but I am undecided."

(8) "Still a cow by the body. I was thinking of various breeds to test it by."

(9) "A cow by the head. The ears are not up high, but they are not drooping like a donkey's. They may be horns."

(10) "It may be a bull from its head."

(11) "I think it's a bull from the neck—an arch on the top."

(12) "I doubt whether it's a bull."

(13) "The shape of the body makes me hesitate between a cow and an ass."

(14) "Distinctly more like a donkey from the whole impression."

(16) "Nose and mouth look like a donkey."

(17) "By the nearest hind leg I should say it's a cow."

(18) "Fore legs are those of a donkey."

(19) "Donkey or pony from the nose."

This wavering went on for thirty-six exposures, during which the subject made mention of bulls, horses, cows and donkeys. At the end he was still a bit uncertain :

"I think a donkey by the way it stands. I would express my decision as five to three in favour of a donkey."

Curiously enough, indecision was never attributed to any fault of the pictures; it was the accurate and decided subjects who made adverse comments on them.

(f) *Associations.* Subjects in whom the various subjective tendencies were strongly marked generally had so abundant a flow of associations that they were unable to report them all:

"I just get a flash, and associations crowd in."

"Vague fleeting associations passed very quickly—figures in *Pilgrim's Progress*—one of a man with a whip trying to pull a donkey along, though I can't see its connexion with the picture." (No. 6.)

"Associations so vague that I can't tell them to you."

Subjects in whom subjective factors had less sway, on the other hand, had usually few associations.

It is interesting to note that associations very often ran along some definite line, such as localities visited, or pictures seen, or events of childhood. Thus, of seven associations detailed by one subject, five were definitely of places. In another case, of nine associations six were definitely remembered events of childhood.

Associations might be vague and general, or individual and vivid. They were nearly always a source of pleasure. In a few cases self-projection occurred with them:

"I felt myself actually showing the picture book (an association) to my niece." (No. 5.)

"This is a picture, but I was thinking of an actual bear dancing, not so much of the picture here. I felt it to be ludicrous to watch a bear standing up like that."

Like imagery, associations were sometimes a source of error. This occurred when they were so strong that they replaced the actual percept. I quote from a few cases of this kind:

"It was associated very vividly with a station in Devonshire. I think I may have imagined the steps from that. The associated station took the place of this one immediately." (No. 2.)

"I find it hard to distinguish between what is seen and what is associated."

"I saw a peculiar expression on the dog's face, and then some association interpreted it as contempt."

Associations had, to some extent, a reciprocal relation to inferences. Those subjects who were able to proceed by inference did not usually get many associations, whereas those whose thoughts were one long train of associations rarely made inferences at all.

(g) *Self-projection.* In a few cases the pictures were seen as real things, and the subjects projected themselves into the events and became

344 *An Experimental Investigation of Perception*

actors instead of onlookers. I have already quoted at length (page 338) from the most pronounced case of this kind, and add a few quotations from other subjects:

"There's a kind of excitement about it that gives me pleasure."

"It reminded me of a square in Copenhagen. I was looking at it from my hotel window. It was very pleasing."

"I am in the street but not frightened. He looks quite nice and tame. I am prepared to believe the master is near. I am some little distance away. He's not looking in my direction, and doesn't see me." (No. 13.)

Self-projection occurred only rarely with adults, and with children I think it is quite impossible to say whether it occurred at all, because I am not sure that they understood such questions as "Did you feel as though you were there?" or "Did you seem to be in front of a real thing?" although they answered "Yes" or "No" to such questions quite readily. Moreover it was easy to make them change their answers. A few cases occurred where the projection was not quite complete—thus, one subject reported of the station picture that although she was not actually there she was near enough to feel the excitement of the crowd.

In all cases of self-projection, of course, the picture was seen as reality, but the pictures might also become real without the subject projecting himself into it. I quote different cases:

"I think on the whole it is a real train. Any picture would do that. But I am certainly not in it." (No. 1.)

"It isn't a picture. It's a real station—the Quai d'Orsay." (No. 2.)

"It is a pure picture."

"I don't know whether a picture or real. I wasn't concerned with that question, and it didn't arise."

This question has been investigated recently by Mr Aveling¹, whose results show that pictures were perceived by his subjects in three different ways:

(a) As spots of colour—37 times.

(b) As pictures (where the object of thought was the picture *plus* the thought that it pictured a thing)—282 times.

(c) As symbols (where the object of thought was a thing which the picture represented)—295 times.

Compared with my own observations, class (c) here seems to be abnormally high in numbers, for I found such cases to be quite the

¹ "The Relation of Thought-Process and Percept in Perception." *This Journal*, iv. 211 ff.

exception. However as these results were obtained from a very small number of subjects they might be considerably modified if a larger number were investigated. I agree with Mr Aveling when he says (p. 227): "The same picture may be symbolic for one observer and asymbolic for another....The explanation of this phenomenon would seem to lie in the facility with which a symbolic subject assimilates, in the perception, previous experiences of many years anterior date."

(h) *Improbability*. Some subjects were influenced by the improbability of their answers:

"The whole idea was of a sweep carrying brooms. Then I thought of the light blue suit, and so inhibited the idea." (No. 3.)

Others were not influenced in the least in this way. They might mention the fact that what they were saying was improbable, but that did not seem to them a sufficient reason for rejecting it.

(i) *Influence of the first idea*. The first idea was often of very great importance in determining the subsequent course of the interpretation, and frequently led subjects astray when it was wrong. Such a subject, an ex-schoolmaster, on seeing picture No. 14, took it to be some school children drilling. In the succeeding flashes he saw, under the dominating influence of this idea, the regularity of the lines and the division into squads. This illusion held during forty-four exposures when I told the subject he was wrong. He had had no doubt about the matter.

Other subjects seemed to lay no more importance on the first interpretation than on any other. They might change completely at the second flash, or even after a large number of flashes they showed themselves just as open to modification as at the beginning.

In concluding this section on subjective factors it is not an easy task to draw any general conclusions from such complex results. But the five subjects who had what I have called a 'self-active' imagery possessed many other characteristics in common. All of them made frequent subjective additions. Four of them spoke very often of objective changes in the pictures. All had a strong flow of associations. Four were fairly undecided. None made really good progress along one definite line. All were drawn passively to outstanding features of the objects, though they showed activity of intention in the fore-period. Inference was weak in four. None was influenced by improbability, and all seemed to receive a strong impetus from their first interpretation.

There was also one subject who showed many of the same characteristics as the other five, but who did not make mention of any image changes. Whether these took place and were overlooked, or whether they were altogether absent, it is impossible to say, but his connexion with this group seems a very close one.

Other factors appeared in very different arrangements among the remaining subjects. Thus, those who analysed the pictures carefully were generally those who made the best progress, but there are striking exceptions to this statement. Other conclusions I have already indicated.

I did not investigate all the subjects with equal thoroughness, so that among the thirty-two there were several of whom I can say very little. But it seems justifiable to separate a small group with marked subjective tendencies, and another class with a more objectively accurate and exact attitude. A large number of subjects were hard to classify, for they did not possess any strongly-marked characteristics.

(iii) *The Perception of Colour.* The experimental conditions under which the investigation was carried out were not calculated to favour accurate colour perception, but several interesting points appeared which are worth mentioning.

In a few cases colour was commented on even before form, and aesthetic judgments were then often made:

"That's in colours."

"I found the attractiveness of the colour, especially of the sky, almost a disturbance at the moment of recognition." (No. 1.)

"I found the strong light and shade very confusing."

"A cheap process."

"The sky is a horrid unpleasant green colour." (No. 1.)

Other subjects showed a marked indifference to colours, and only mentioned them fairly late in the experiment, after other matters had been investigated and settled. One subject correctly saw green in the engine at the first exposure, but made no mention of the blue sky, which was very bright and prominent, till the fifty-eighth exposure. Another missed a very prominent bluish-green colour in picture No. 7 till the forty-fourth exposure. I then told him to look in a certain direction, and he at once saw the colour.

When I used the shortest exposure, about one-hundred-and-thirtieth of a second, colours tended to disappear altogether with the majority of subjects, except where the colours were very highly saturated, or where the subjects were interested in and attracted by them. Thus, if I began an experiment with a coloured slide at the shortest exposure, I often

had the report "Not coloured." This occurred almost always with pictures 2, 3 and 4. In the case of picture No. 2, I showed it to ten subjects at this quickest speed, and eight of the ten reported that there was no colour in it. As I slowly lengthened the time of exposure this judgment generally remained unaltered for a long time; sometimes, indeed, colour was not mentioned till the picture was permanently exposed. Occasionally, a colour seemed to be just on the margin of perception. Thus, in picture No. 3, at the normal times of exposure, a subject saw blue in the third flash, but not again till the fourteenth, when he specially looked for it.

Fatigue had great influence on colour perception. This was well shown by one subject with whom I was working fairly late at night. He failed to see any colour in No. 3 at normal exposure time, though I asked him about it. As he was tired we did not finish the picture that night, and on continuing it next morning he saw at once that it was coloured.

Although I did not use any quicker flash than one lasting one-hundred-and-thirtieth of a second, yet even at that speed some subjects continued to see almost as much in the first flash as they had previously seen with the normal time of exposure. On the other hand, I had one subject where the time threshold for colour perception seemed to be exceptionally high.

(iv) *Feeling Tone.* These experiments were not very favourable to a satisfactory investigation of feeling tone. As Kuhlmann says: "Pictures as such have not the interest and emotional colouring that belong to objects. Such emotional colouring, when it takes possession of consciousness, brings with it the visual imagery with which it is connected. Again, in so far as meaning and interpretation is read into the picture at all, so far, of course, the picture ceases to be what it is and becomes the object. The picture to this extent creates the tendency to substitute the imagery of the object in its natural setting¹." One subject said: "Feelings are an after thought with me. I have to recall the picture."

At the outset the shortness of the flash was a disturbing factor, because subjects felt somewhat hurried, and their chief attention was given to the subject-matter of the pictures. Owing to the pre-eminence of this claim the emotional tone was apt to be ignored by the less practised observers. The shortness of the flash irritated quite a number of subjects:

¹ F. Kuhlmann, *op. cit.* 415-6.

"I have a feeling of discomfort because it is so short."

"I am aggravated because the picture disappears so quickly."

"The instantaneous exposure is aggravating."

One or two subjects, on the other hand, were stimulated:

"Speed gives piquancy to it."

"The shortness of exposure does not irritate me. It arouses my curiosity."

Subjects who experienced some difficulty in solving the pictures often developed disturbing feelings of self which rather added to their inability:

"I am beginning to feel irritated at my inability to decipher it."

"I have a nascent feeling of shame that I have to make two great corrections."

(The corrections, however, were not 'great'.)

B. *Experiments on Children.*

I now pass to an examination of the results obtained with children. These were kept separate from the rest, so that the two groups might be the more easily contrasted and compared.

The three schools were chosen because of their typical differences, and at each I worked with a group of children of the average age of twelve years, and also at the two elementary schools with a group whose average age was six years. Unfortunately I was unable to get children of this age in the Secondary School.

Stern has worked out with some completeness the mental development of children, and the modifications due to age and sex¹, but my experiments are much less complete as I was working at a more general problem. Hence I confined myself to two ages in order to see generally in what direction changes occur with the advance of age. Moreover, I did not use many girls in these experiments. But it is possible that this same method of investigation might throw light on the question of mental development in school children, if it were applied uniformly to groups of both sexes at many different ages.

(i) *Six years of age.*

I will deal first with the results of the youngest children.

¹ "The time from seven to ten years is for boys a period of strong development, whilst girls during the same period virtually remain at a standstill and their reliability even recedes; between ten and fourteen years girls, by rapid development, recover the lost ground and even overtake the boys a little, whereas the latter show no improvement. Therefore the difference between boys and girls is greatest at the age of ten." Stern, "Die Aussage als geistige Leistung und als Verh rsprodukt," *Beitr. z. Psychol. d. Aussage*, Erste Folge, 291-2.

(a) *An elementary school in a very poor district.*

In this school, where the children were from the poorest homes, and of parents of a very inferior type, I worked with four normal children, two boys and two girls, whose average age was a little over six years.

The most outstanding feature of the boys' results was a very pronounced tendency to add imaginative detail. Nothing seemed too grotesque or too impossible to apply to any picture. This tendency was not so marked in the girls, but in the case of the boys there seemed to be no ability to distinguish between what was accurate and what was inaccurate, between what was seen and what was imagined. I will quote from one case:

- (1) "A house and windows and door. A little boy peeping out of the window."
(Picture No. 1.)
- (2) "A house and a bedroom."
- (3) "A little boy outside."
- (4) "The little boy is hiding. I saw him peeping."

This continued until the eleventh exposure, when I told him it was not a house. He continued:

- (12) "It was a house."
- (13) "A train. And a woman in it peeping and waving her hand."

In the Queens' College picture this same subject saw a little boy, and described his "hat and blue sailor coat." When I suggested there was also a woman he agreed, and added that she was waving her hand to the little boy, and he was waving his hand to her. At the end I showed him the picture, and asked where the woman was. He replied very confidently: "She's in her house."

Many of the boy's replies seemed to be faint memories of past experiences. Thus, he described the bear as a "woman," and added the curious description that she was deaf and dumb. When I asked him the reason for this, he replied that "she carried a pole." On further questioning, I found that he had once seen a deaf and dumb woman with a long stick, and undoubtedly the idea had come from this recollection. And again, his first answer in the station picture was: "Little boy. Green hill. Stick in his hand." All these three phrases had been used by him previously in other pictures and undoubtedly came back to him from them.

The other boy showed the same tendency. In the picture of the child (No. 4), he described shoes, stockings and coat quite readily. When I showed the picture at the end, and asked him to point to these things, he touched the bottom of the glass screen vaguely, and said they were

"down there." In the same picture he said there was "green grass, and in the bear picture he spoke of a "train." These answers, too, were undoubtedly due to the other pictures. Some of the additions cannot be explained in this way. Thus, when he saw a house in the bear picture he added quite spontaneously: "There's a house with windows, where the little boy lives, and he's got a father and a mother and a baby and a cow."

Sometimes these boys developed a kind of 'additive' description of some object, which consisted of adding together all the possible qualities of such an object, utterly regardless of objective accuracy. Thus, in picture No. 6, this second boy described the central figure, after an exposure of one second, in the following way: "A man. Red. Green hat. Black shoes. Black trousers. Red scarf. Blue waistcoat. Blue eyes. White ears. White nose. White mouth."

The two girls were much more exact and reliable than the boys in describing what they had seen and in repeating what they had said, though one of them showed a passive tendency to repeat former sayings regardless of their meaning. Thus, in the child picture, she first described it as a "man" and gave several qualities, including "whiskers." When she discovered it was a child, she corrected her mistake, but added the other features, including "whiskers," in full, and only withdrew the impossible ones when I drew her attention to them.

It would be interesting to discover the cause of this indifference to a strict line between reality and imagination. It may be that both boys had a 'self-active' imagery, but this explanation would not cover all the answers. I think, rather, that the problem is one of environment. The mistress of the school agrees that the peculiarity I have described is far more common among slum children than among those from better class homes. She suggested that the explanation is that these children have to give answers at home which will please their parents. The strict truth of the answer is not an immediate concern. If the answer displeases, the child is punished. Hence with a stranger he is willing and even anxious to see what he thinks he ought to see, and this brings about an attitude favourable to 'creative imagination.'

This explanation also accounts for the very marked susceptibility of these children to accept suggestions quite readily, and even to add descriptive details to the objects suggested. The two boys and one girl did not show the least inclination towards caution, but caught up each suggestion I gave immediately and fully. When I suggested a dog in the station picture, one boy said he had seen it and that it was barking.

I asked one of the girls if she had seen a swan in the Queens' College picture, but she did not know what the word meant. I also told her there was something going right across the picture (meaning the bridge), which she had not yet seen. After the next flash she showed how very passively suggestible she was by saying: "It's a swan up there."

The other girl was more independent and critical. She refused suggestions often, and only accepted them when there was some justification of their probability, and at the end she was careful to correct her mistakes.

These subjects had no problems and showed no indecision. In the donkey picture one boy mentioned six animals successively, and each was given with an air of confidence and finality. There was never any postponement of decision, no matter how incompletely the picture had been seen. Each judgment, when made, was final for the time being, though if it had to be abandoned another would be offered with the same finality. One of the boys interpreted picture No. 12 as, in turn, a mountain, a ladder, a motor car, a little boy, a chair, a tram, a bus, and a picture. There was no hesitation about each judgment, no balancing of alternatives, no consciousness of the need of deliberation.

The pictures were looked at as a whole, and there was no analysis¹. Practice did not seem to bring about any modification of method, or to develop any caution. Inferences were quite absent. Associations were rare, though this was partly due to the inability of the children to recognize them as such, and to their neglect in reporting them.

As was to be expected from the experimental conditions, the perception of colour was very difficult, as was also the exact localisation of the colours seen for so short a period. This accounts in some measure for the many mistakes made. The child was described as having a "blue coat and black hair," whereas the blue was round the child, and the hair was brown; the train was said to have "grey" carriages, whereas in reality they were brown.

So far as it is safe to distinguish between the sexes in such a small number of cases, the girls showed some superiority over the boys in several ways. Thus, if we take the average number of facts correctly observed in each picture and divide the two boys from the

¹ As Binet says, "What is lacking in the child is that he does not possess to the same degree as ourselves the power of analysis." "Perceptions d'Enfants," *Rev. philos.* xxx. 592. In the same article Binet relates that a child of one year and nine months was able to comprehend the drawing of a horse, but even at four years was still unable to understand the drawing of a nose, or of an eye.

352 *An Experimental Investigation of Perception*

two girls, we find that while the boys only observed 7·1 facts in each picture, the girls observed 10·9, and that whilst the girls made only 8·0 mistakes in each picture, the boys averaged 9·6. In describing correctly the positions of objects, the girls also had the advantage, the figures being 2·1 as against 1·5.

(b) An elementary school in a good district.

At the other elementary school, where the children were of a higher social status and from more cultured homes, I had five boy subjects whose average age was six years.

Here there was very little tendency to add imaginative detail. It appeared occasionally, as when one boy called the donkey a "horse" and then added that there was a cart fastened to it; and also when he said he saw telegraph wires on both sides of the railway lines. But the first of these additions was afterwards corrected quite voluntarily.

Suggestions, too, were not accepted so readily and were often resisted quite firmly. In the case where a mistake had been made (such as calling the bear a man), these children, when told they were wrong, did not make their correction immediately and rashly as had done the others: they looked again. One boy, when I told him he had made a mistake, looked a few more times and then said: "I don't know. I don't think I shall get it."

In many particulars there was close agreement, of course, between the children of the two schools. The method of solution was the same: that is, the whole picture was looked at each time, and no analysis was attempted. Decisions were made at once, and the subjects were soon satisfied without making very close examination. Alternatives were not expressed, and a judgment made with confidence at one moment was often cast aside almost immediately for a contrary one, which would be given with equal certainty.

But the superiority of this group over the previous one was marked. Two children made the inference that as there was a shadow the sun must be shining, and another said he saw a steeple, and so argued a church from it. Reasons were often given for statements: thus, in the station picture one child naïvely argued there was a bride, because he saw a "lady with a veil and baby"! He afterwards corrected himself and said she could not be a bride because she had a bonnet on and had no roses. To another boy the donkey was a cow "because it had horns," then a horse "because it had reins." Another saw what he thought were chimneys at the bottom of the bear picture "as though the picture

was upside down." This tendency to give a reason for a statement, even if the reason is a wrong one, shows a much higher mental process than do the unreasonable answers given by the first group.

The language was very similar in the two cases, though at the second school more verbs were used, such as "curves" and "walks"; and longer sentences were formed.

Some of the subjects had a great power of seeing very small things: thus, the engine driver, who can just be distinguished looking out of the engine, was first noticed by one of these children. Colour perception was again rather poor. One of the five refused for some time to change his opinion when I told him he was wrong. Another told me before one of the exposures that he had made up his mind where to look; this, however, only occurred once. The complex picture No. 6 was thus interpreted by one subject: "He's dabbing the brush in her face on purpose, and the old man is telling him not to do it." Associations were often mentioned and appeared to be much more frequent than at the first school.

The average number of facts correctly observed by each subject was 10·5, and the number of mistakes 6·15. The average number of positions given correctly was 3·35. Hence these boys showed marked superiority over the two boys, and are slightly better than the girls of the other school.

We may conclude generally that this group showed much more reliability and activity than the other. There was a more critical attitude, a greater accuracy and much less sway of subjective tendencies.

(ii) *Twelve years of age.*

I now pass to the older children, a group of whom came from each of the two schools just described, and a third group from the — Grammar School¹.

(a) *An elementary school in a very poor district.*

In the first elementary school, with children of the poorest class, I had seven subjects—four boys and three girls—whose average age was twelve years and four months.

There was greater evidence of activity than among the younger children at the same school. This was shown by the occasional refusal

¹ This school is well known for its adoption of modern methods of teaching, and for its educational experiments. A spirit of independence among the scholars is more marked than at other schools.

354 *An Experimental Investigation of Perception*

to accept the suggestions I made. Thus, one boy saw something white coming under the bridge in the Queens' College picture, but my suggestion that it was a swan did not influence him, and he was content to leave it with a non-committal "I can't tell." Another boy showed quite commendable caution in the Brittany picture: he agreed it was "something like a window," but he refused to go any further without more evidence. Occasionally, some of the subjects decided what to look for in the fore-period, though this was exceptional. A few inferences were made, too, and at the end some of the subjects enumerated their chief omissions, and corrected the most obvious of their mistakes.

But on the whole these subjects must be described as largely passive. The first idea that came to them seemed to determine very largely their subsequent answers. Thus, in picture No. 6, the male figures, which were seen as boys in the first flash by one subject, remained boys throughout. Many more suggestions were accepted than were refused. The following answers to three successive flashes of picture No. 2 show how completely susceptible this subject was to every suggestion:

- (8) Have you seen the dog? "Yes, near the little boy on the right."
What colour is it? "I didn't quite see."
- (9) "The dog looked brown, with two patches of white
on its front feet."
Is it like any dog you know? "No."
- (10) "It seemed like Mr Brown's dog, with a short
tail."

There was a clear case in this group of 'self-active' imagery. In the picture of the train he made the following reports (I omit details):

- (1) "A little boy crossing the lines at the left side, waiting till the engine passes. He's waving his hand."
- (2) "It looked as if the stoker had just made up the fire—smoke was coming out of the chimney on the side where the boy was. The boy was waiting on one side, and the engine was nearly past him."
- (3) "It looked as though people were popping their heads out of the window and waving their hands at the little boy."
- (6) "The engine seems past the little boy now, and he's just going to step across the lines."

Objective change was present in several of the subjects, as the following answers indicate:

- "This is a different boy." (No. 5.)
- "The train is nearer and bigger every time." (No. 1.)

Colour perception was still poor and inaccurate. Four of the seven did not mention the blue circle behind the child's head, though the colour was very striking: they may have seen it, but it evidently did not seem of importance to them. There was also a vagueness about colour answers which indicated inaccurate localisation. Thus, in the train picture, one said there were "red, blue and white flowers on the floor," whereas the stones on the left were a vivid reddish-brown, and the hedges and ways were a bright green. Many said the child had "dark" hair and a "blue" coat, as did the younger children at this school.

If we compare the girls and boys in the group, the former show a decided superiority in several ways, a result which is in agreement with many other observers. Thus the average number of correct facts discovered in each picture was 12·58 in the case of the boys, and 14·5 in the case of the girls, whilst the latter only averaged 3·58 mistakes, and the former made 7·0 mistakes.

The girls gave more correct reasons for their statements than did the boys. They also seemed better able to discover the explanation of the complex pictures. Thus, of the station scene, one girl said: "The man looks like the woman's son. He may be going away." This is the correct explanation, and was only given very rarely even by adult subjects. Picture No. 6 was also 'explained' very creditably by the girls, whereas the boys rarely solved or attempted to solve these complex puzzles.

Girls had more associations than boys, and made better progress in getting out the details in succeeding flashes. They also showed greater power of analysis. The number of subjects is too small to warrant any certain generalisations being drawn from these differences between the two sexes, although the distinctions were fairly consistent in the cases observed.

(b) *An elementary school in a good district.*

At the second elementary school I had six boys as subjects with an average age of eleven years. This lower average was due to the fact that three of the boys were only about ten years of age.

Mental activity was more generally noticeable than in the group just mentioned at the first school, and was shown in several different ways. Thus one boy, the youngest of the group, constantly worked down to causes:

"His hair is rather rough, as though he is just up." (No. 5.)

"His lips are only a pale red, as though not completely healthy." (No. 5.)

356 *An Experimental Investigation of Perception*

Another subject said of the same picture :

"He is closing his right eye, as though somebody has thrown something at him, or is going to hit him."

Probable things, not yet seen, were specially sought. Thus, in the picture of the donkey, one subject said :

"There ought to be some sky. I haven't noticed any."

After the next exposure he added :

"I couldn't see any sky. The cow seems to be on a hill, and the ground rises up behind."

Another subject noticed that he had not seen a signal in the train picture, and so looked for one.

Two subjects, on the other hand, were amazingly uninquisitive. They were satisfied after a very few flashes that they had seen everything, and would keep saying at the end of subsequent exposures, as though tired of the picture : "That's all." These subjects accepted suggestions as readily as any in the previous group.

Introspection was more exact at this school than at the first, as the following examples show :

"I think I noticed the colour first. I saw it was white. Then I thought a white cow." (No. 11.)

"It reminded me of going from Cambridge to Yarmouth. I felt in the train but it was still. I also thought of my brother and sister in the train." (No. 1.)

Colour perception was again weak. Two missed the colour altogether in picture No. 5, and all subjects made mistakes.

There was an excellent example at this school of a subject showing his predisposition to one special aspect of the pictures. In this case it was to geometrical notions and concepts, and the boy compared lengths, proportions, sizes and angles in almost every picture :

"It did not seem to have long legs compared with the body." (No. 13.)

"The sides of the road seemed to vanish quickly." (No. 13.)

"The elephant seemed about one and a half times as high as the man."

When I asked the headmaster to place the boys in the order of their ability in mathematics, he gave this subject the top place, without knowing anything of these results.

There were no cases of self-active imagery, and objective change was reported by one subject only :

"I have a different idea." (No. 13.)

"I get a different view." (No. 1.)

The average number of correct facts discovered in each picture was seventeen in the case of the three older boys, but if we take all the boys together the average was only 14·2. These results show a significant superiority over the average of 12·58 gained by the older boys at the first school. (Unfortunately I had no girl subjects in the second school.) The average number of mistakes made in each picture was 5·65 for all the six subjects, this again comparing favourably with the other group. More correct reasons for statements were discovered, and more exact positions of objects were stated by the second group than by the first; also, more associations were mentioned.

Besides this general superiority which is evident in the second school, there is also evidence of more individual variation, and generalisations are more difficult to make. Some subjects showed a fairly independent active attitude, as I have shown, whereas others were amazingly passive.

In this group, too, the subjects had the benefit of a wider experience than in the other, and seemed better able to deal with the strange and unusual. Although the school is much further away from Queens' College, two of the four to whom the picture of the College was exposed recognised it at once, a third knew it was a Cambridge College but could not name it, and the fourth recognised it correctly at the end.

(c) *A Grammar School.*

At the — Grammar School I examined nine boys, whose average age was twelve years and two months (varying from eleven years and ten months to twelve years and six months).

Two striking differences from the results obtained in other schools may be mentioned at once. The first was the caution used in describing the nonsense picture (No. 16). From the children in other schools, as indeed from many adults, I got many very elaborate and extravagant interpretations, as the following:

"Some ducks walking about on land."

"A picture of St George and the Dragon."

"A boy on a donkey's back, and some more children standing at the side."

And to these interpretations further details were often subsequently added. But at the — School the boys gave a very meagre and restricted interpretation to the picture. The following answers were returned by different boys, all after the first flash:

"A whole lot of jumbled figures with a box in the middle. Done in blue ink. All dancing round a box. Little ink smudges."

358 *An Experimental Investigation of Perception*

"A pattern, isn't it? Blue colour."

"Like a landscape puzzle. Green colour."

"I don't know what it is. Blue things. Might be Japanese writing. It goes in all directions."

"A lot of blots. It reminded me of when I put ink on a lantern slide and showed it on a sheet."

"Black figures on a white background."

These answers show great objective accuracy, and the ability to keep very close to the bare percept—an attitude which is the antithesis of the tendency to subjective addition so clearly marked at the first elementary school.

The second difference was the very systematic and careful examination of each picture at the end of the experiment. Corrections were made with great accuracy, and omissions were noted, the subjects showing a very general absence of bias and prejudice. I quote two examples:

"Oh! it's a bear performing in the street." (The subject had previously thought it was a man.) "There are houses and shadows—not a sledge. A chain, muzzle, trees either side, big buildings, some very tiny vehicle at the end of the street. The white thing on the bear is a muzzle. The head is shaggy on the top. There's a chain attached to the pole, and I believe a bit of chain attached to the paw. There's a little boy in a white jersey and black trousers looking at the bear; he's leaning against the wall of the house nearest to me on the right. The trees extend the whole way up."

The second is the picture of Queens' College:

"There are no swans. It's a College I think." (This subject was not a Cambridge boy.) "At the far end are two boats and men rowing. On the left a low sort of wall. A clump of trees. Stone buttress pointed at the top. Behind the bridge and on the right a part of the building sticks out. A pointed roof. Three chimneys. Windows deep set and round. Buttress supports bridge. Brick house. Two rows of windows. At the end of the picture there are two windows at the top, then three windows, then a division, then three windows. They are square and have bars on them. I can see the roof of the part with round windows. It is black and has garrets."

Children at other schools generally ignored the mistakes they had made, and some would stare at a picture, exposed to them at the end of the experiment, without adding a single correction.

Suggestions were most often refused quite decisively, though they were occasionally accepted when there was some ground for their probability.

Several boys looked out specially for objects which were likely to be found, but which they had not yet seen, and chose special parts of the pictures for examination:

"I haven't seen the tender yet." (No. 1.)

"There ought to be some posts nearer me." (No. 1.)

"I am going to see if there are houses on the same side as the pavement."
(No. 13.)

"I was going to see what the woman was sitting on." (No. 12.)

One boy is to be distinguished from the rest as showing much less care and caution. He had been attending the school for a short time only, though this, of course, may only be a coincidence. He did not correct his mistakes at the end. He went far from the actual percept. He said there were "about six people" in No. 6, and "a cat" between the man and woman. In the bear picture he saw "a crowd of people on the right." He made generalisations on very insufficient data. In No. 6 he said the figures were "dressed in bluish stuff." In the station picture there were "no porters or officers about." However, he made inferences with fair ease, and of the Queens' College picture he said very shrewdly: "The trees form a very deep grove, and make the place very dark. The windows are not much good because the light doesn't get to them very well. The river is not very wide."

Complex pictures were not very well solved as a rule, and it was evident that the main interest was not in the hidden story of a picture, but in the observable details it contains. One boy was remarkably clever in discovering the important features of a picture at once. He seemed to be able to ignore instinctively what was unimportant, and to concentrate on the significant parts.

Language was more complex and varied than among the other groups, and expressed finer shades of meaning. There was one case where a subject added so many qualifications to his answer that I could not write them down quickly enough.

The average number of correct facts discovered by these subjects in each picture amounted to 19.4, and the mistakes numbered 3.15. These numbers show a considerable superiority in discovering detail and in caution possessed by this group as compared with the others. Associations, too, show a marked increase, evidence, it may be, of a fuller experience. The ability to state the exact positions of objects and to give the correct reason for things was also possessed in a higher degree at this third school than at the others.

Objective change was remarked upon by one subject, but subjective addition was quite absent. The power of analysis was present to a very much more marked degree than in the other groups.

To sum up, we may say that the children in this group showed evidence of remarkably advanced mental powers in dealing with the

360 *An Experimental Investigation of Perception*

problems before them. They were systematic in the fore-period, and careful during the experiment. They showed caution against subjective errors, and independence of judgment. They based their answers on good reasons and did not lightly dismiss alternative possibilities without good cause. In all these respects they approached the characteristics of adults, and showed a very pronounced superiority over the other two groups of similar age.

This fact of variation from school to school is of importance in psychological investigation, and offers possibilities of further research. It is easy to see that the second of the three schools occupies a position midway between the other two. At the first school the older children did not seem to have advanced very far, in some respects, over the younger children at the same school.

The figures which I have already given may be set out in tabular form for greater ease of comparison :

School	No. of subjects	Age	Av. no. of correct facts in a picture	Av. no. of mistakes in each picture	Av. no. of correct reasons for things	Av. no. of positions correctly described
1. Elementary (poor district)	2 girls	6	10.9	8.0	0	2.1
	2 boys	6	7.1	9.6	0	1.5
2. Elementary (good district)	5 boys	6	10.5	6.15	0.15	3.35
3. Elementary (poor district)	3 boys	12	12.58	7.0	0.25	4.75
	3 girls	12	14.5	3.58	0.4	3.8
4. Elementary (good district)	3 boys	10	11.4	3.8	0.1	4.2
	3 boys	12	17.0	7.5	0.58	5.25
5. Grammar	9 boys	12	19.4	3.15	0.4	7.6

(iii) *A mentally defective child.*

An interesting case presented itself at one of the schools, where I had a mentally defective child as a subject. This was a boy of eight-and-a-half years of age. He was inferior to children much younger than himself, and was very open to suggestion, but his chief fault was his inconsistency. He showed no power of making gradual progress over an extended period, no ability to solve a problem by degrees. Lack of memory largely accounted for this weakness, though not entirely, I think. His attention would wander off in a distressing way. When left to himself he merely gave enumerations of the things he had

seen and imagined. Thus, in a picture of two bulldogs (not included in the descriptions given earlier), I did not interfere at all with him, and his answers were :

- | | | |
|---------------------------------------|-------------------------|--------------------------|
| 1. "Two dogs." | 12. "Sun" (imaginary). | 22. "River" (imaginary). |
| 2. "Two dogs." | 13. "Moon" (imaginary). | 27. "Dog's nose." |
| 3. "Sitting down." | 15. "Ears." | 28. "Other dog's nose." |
| 5. "Dog's legs." | 17. "Head." | 31. "Pavement." |
| 7. "Tail at the back"
(imaginary). | 20. "Sky" (imaginary). | 32. "Road" (imaginary). |
| 10. "Road" (imaginary). | 21. "Star" (imaginary). | 33. "Sea" (imaginary). |

It will be noticed how one answer often suggests the next (12 and 13; 20 and 21; etc.). Others are probably vague memories of previous pictures: the "river" from Queens' College, the "road" from the bear picture, and so on.

When I asked this subject questions in order to make the reports more explicit, I was soon in a maze of contradictions and inconsistencies, for he would accept even the slightest suggestion and then promptly forget it in favour of something else.

This research is incomplete at many points, but the problem is so vast, and involves so many factors, that this fault could not be avoided in the time at my disposal.

In conclusion, I should like to express my very great indebtedness to Dr Myers, who first suggested the experiments to me, and who has helped me unceasingly during their progress. My thanks are likewise due to Dr Rivers and to Mr E. Bullough for much valuable criticism and advice; to Dr Rouse, Miss Wing, Mr Haynes, Mr Inglis and Mr Mullett for their courteous help in the school experiments; and also to my numerous subjects, who often visited the Laboratory at great personal inconvenience.

5. CONCLUSIONS.

The process of perception begins with an immediate interpretation of the object, and this is strikingly uniform in different subjects. It is followed by an analysis of the object, and, in this, very marked individual differences emerge.

Among adults, persons of scientific training show advantages in the method of analysis, and are generally better able to direct the process systematically and carry the analysis further than are other subjects, though there are exceptions to this statement.

362 *An Experimental Investigation of Perception*

Some persons with strongly marked subjective tendencies behave very consistently in many respects, and it is probable that the cause of their similarity is to be found in a 'self-active' imagery, which plays a more important part than the actual percept. There are also subjects who show very little tendency to be influenced by subjective factors, and with whom the image has little or no importance.

Among children, perception becomes more complex as age increases. Children of six years of age have no power of analysis, and see the object as a whole. They cannot balance alternatives or postpone decisions. They are very passive to suggestions. Children of twelve years of age show a greater power of analysis, a more active mental attitude, and an improvement in the discovery of detail. Compared with the younger children there are greater individual differences.

Children of the same age from different schools vary in a marked way. At a secondary school children of the age of twelve have already many of the powers and methods of adults. At a 'slum' school they are much inferior and even show some of the characteristics of the youngest. Mental activity is as marked at the first type of school as passivity is at the second. An elementary school of a good type shows characteristics midway between the other two.

THE COLOUR PERCEPTION AND COLOUR PREFERENCES OF AN INFANT DURING ITS FOURTH AND EIGHTH MONTHS¹.

By C. W. VALENTINE.

- I. *Purpose of experiments.*
- II. *Results of previous investigations.*
- III. *A new method of experiment.*
- IV. *Results of experiments.*
- V. *Discussion of results.*
 - (a) *The presence of colour sensations.*
 - (b) *Colour preferences.*
- VI. *Experiments by the 'grasping method.'*
- VII. *The method of grasp and reward.*
- VIII. *The use of the hands in grasping.*
- IX. *Summary of results and conclusions.*
- Appendix. Determinations of the brightness values of colours used.*

I. *Purpose of Experiments.*

THE experiments here described were begun upon my little boy when he was just over three months old. I wished to discover what colours he preferred at that age, before their 'natural' effect could have been materially affected by any disturbing associations. In the course of the experiments I was also on the look-out for evidence as to the development of the colour sense.

II. *Results of Previous Investigations.*

The most important recent investigations known to the writer are those of Baldwin², Miss Shinn³, McDougall⁴, and Myers⁵.

¹ An abstract of this paper was read at the meeting of the British Association for the Advancement of Science, Birmingham, Sept. 1913.

² *Mental Development in the Child and the Race*, Chap. III.

³ *The Development of the Senses in the First Three Years of Childhood*, 148.

⁴ "An investigation of the colour sense of two infants," *This Journal*, II. 338.

⁵ "Some observations on the development of the colour sense," *Ibid.* 353.

Most of the experiments referred to by these investigators were performed upon infants between the ages of six and twelve months, and with the exception of Baldwin, the writers show considerable agreement as regards the colours preferred. Myers found that light grey was preferred to dark grey, but that in spite of this partiality for brightness, yellow was preferred to white. McDougall, whose investigations were the most extensive, concludes that, in the sixth month, red, green and blue were all preferred by one of his children, to greys of the same brightness and even to white, but that no one of these three colours, red, green and blue, was markedly preferred to the others. His figures, however, suggest that red tended to be preferred to blue by one child during the sixth month¹, and that yellow was slightly preferred to red by another child of seven months².

Miss Shinn, reviewing the evidence of several investigators, concludes that during the second half-year of life, red, yellow and orange are the most attractive colours, but she thinks that white is even more attractive³.

Baldwin's results are somewhat different. They suggest at first sight that blue is slightly preferred to red by his child. But Baldwin himself shows that his child was less inclined to grasp the coloured papers the further they were from him. Now red was placed 22 times at one of the far distances used by Baldwin—13, 14 or 15 inches—and blue only 16 times; while at the shorter distances of 7, 10 and 11 inches red was placed 8 times and blue 10 times. Thus the placing was obviously in favour of blue. Yet Baldwin masses all the results together for the purpose of calculating percentages. This conceals the fact that, at the long distances, red fares distinctly better than blue.

Thus Baldwin's results suggest after all that red was at least as

¹ The figures are: red chosen 82 times, *versus* white chosen 33 times, when the two were presented together, the scores for blue *versus* white being 17 to 13. McDougall mentions a possible source of error, *op. cit.* 341.

² *Op. cit.* 345. If the scores for and against yellow and red given in Table III are added they give: for yellow 32, against 12; whereas the figures for red are: for 42, against 31. The scores when red and green were presented together are omitted, as yellow was not presented with green.

In considering the long series of experiments summarised in Table II (p. 343), one must take into account the scores *against* each colour as well as *for*, owing to the fact that they were not all presented the same number of times. The *total* scores are, red: for 128, against 67; green: for 107, against 86; blue: for 114, against 88; white: for 96, against 118; grey: for 31, against 117. This shows an appreciably higher *proportionate* score for red.

³ *Op. cit.* 158.

attractive as blue to his child. On the whole, then, the results of these investigators indicate that red and yellow are the colours best liked by infants of this age (as far as this can be judged from the examination of five children), though Miss Shinn concludes that white was liked even more than yellow and red.

III. *A New Method of Experiment.*

I wished to make a test as to the colour preferences of my child W. at an age considerably younger than that of the infants tested by previous investigators, viz. at three months. At this age, however, grasping had not developed sufficiently to make Baldwin's method, or the modified methods of McDougall or Myers, possible. But I convinced myself, by some preliminary experiments, that another method was capable of giving fairly reliable results.

Briefly, the method was to measure the time during which W. looked at either of two coloured wools held before him for two minutes at a time.

The wools were selected from Holmgren's wools for testing colour-blindness. The infant was placed in a comfortable position among cushions in an arm chair. The chair was placed near the window on dull days, but near the middle of the room when there was strong sunlight. The direct rays of the sun never fell on any of the colours. Greater constancy of illumination could have been obtained by means of artificial light, but the variation was but slight, and this very variation provided interesting evidence on at least one point to be mentioned later. I sat on a chair in front of the child and held two of the coloured wools about one foot from his face, in such a position that they would appear against the dark grey background of my coat. The wools were held quite close together for a few moments; then I slowly drew them apart until they were some eight inches distant from one another and equidistant from the central line of vision of W. They were then held motionless. As soon as the child looked at either of the colours, or if he followed one of them with his eyes, I called out its name to my assistant, who was provided with a stop watch and with a record sheet, three vertical columns on which were headed thus (supposing the colours to be red and blue):

Red	Off	Blue
-----	-----	------

The stop watch was kept going continuously. When I called out the name of the colour looked at, my assistant noted the exact second

indicated by the watch, and put it down under the name of the colour on the record sheet. When the child turned his gaze away from the colour, I called out 'off,' and again the exact second was noted and recorded under 'off.' When again a colour was looked at its name was called out. Thus the record would appear somewhat as follows:

Red	Off	Blue
5		
	11	
		21
	25	
		28
30		
	45	

and so on. From such a record it was easy to calculate that red was looked at from second 5 to second 11 (*i.e.* for 6 seconds); that neither colour was looked at between second 11 and second 21; then that blue was looked at from second 21 to second 25, thus scoring 4 seconds, and so on.

The experiment was stopped two minutes from the time at which W. was first noticed to look at one of the colours. He was then played with for a minute or two, after which another two minutes' test was performed, the position of the colours this time being reversed, blue now being on his right, red on his left. The total scores of the two tests were added together, and the colour that scored the higher was reckoned the more attractive.

One further note must be added as to the method of scoring. If W. looked at a wool for less than three seconds, the score of course appeared on the record sheet, but these scores were not included in the totals. Thus a mere turning of the eyes for a moment or two upon the colour does not count. It might indeed be suggested that this in itself indicates some power of attraction possessed by the colour for the child. But such a glance scarcely seems deserving of a score unless the colour holds the attention for at least three seconds. Results are also given based upon the times when the baby looked for not less than eight seconds continuously at one colour.

Now there are obvious difficulties and dangers in such a method, but I think that they are not so great as might be imagined. In the first place it might be thought difficult to be certain whether the baby was looking at the wool or at an object beyond it. But the marked convergence of the baby's eyes, necessary to look at the colours, was of great help to me here. It was impossible to mistake a look directed at the background of my coat for one directed towards a wool.

I gained confidence in the use of the method by practice obtained in a preliminary series of twenty such experiments, during which, when in any doubt, I moved the wool slightly to make quite certain that the baby's gaze was fixed upon it. But the results of these experiments are not included in the following records, for, as every observer of infant life knows, a moving object is likely to attract the attention merely because of its movement. In the subsequent experiments the wools were held motionless.

I do not think that my hands were any serious source of attraction, as the wools were held from behind. Further, W. looked almost invariably at the broad extended *top* end of the skein of wool where no part of the hand was visible. Any occasional errors would tend to spread themselves equally among all colours in a long series. Suspension of the wools upon a screen might have avoided this difficulty, and this plan was followed in some subsequent experiments. But it was not found quite so easy then to follow the child's gaze, and my impression was that the hand method was preferable.

The baby was perfectly free to move his head to right or left, as it rested easily against a cushion. Very occasionally if he seemed to have settled down somewhat to one side, the wools were moved slowly round to that side (when he was not looking at either wool) so that neither might have any unfair advantage of position. There is no record of any occasion on which such slow movement attracted the baby's attention to either wool. Indeed the very fact of his having moved far round indicated as a rule that neither wool was showing much attraction for him on that particular occasion.

A further danger was not overlooked, viz. the possibility of the operator's favouring any colour in respect to position. The writer can only say that he was extremely careful to avoid this and that he did not start out with the object of supporting any particular theory of colour vision.

In the later series of experiments just mentioned, the screen was held steady throughout the experiment. But my final judgment was that with a trained and conscientious observer it is preferable to be able to adjust the position of the colours to any very marked change in the baby's position¹.

The colours used were black, white, red, yellow, green, blue, violet, pink and brown. The red, yellow, green, blue and violet were the

¹ The plan of seating the baby upon his mother's knee was tried, but he obviously did not like this so well, and tended to wriggle more than when seated on cushions.

purest obtainable from the Holmgren wools. Of these the yellow was only moderately intense. As I had not perfectly white wool at hand I used a piece of dull white linen made into a roll of about the same size and shape as the wools.

I should like to have included grey and orange. But as I used the method of comparison in pairs (each colour being compared with each in the course of the experiments), the addition of one further colour would have meant eighteen further experiments and, as it was, the series was prolonged as long as seemed advisable. For the baby tires of the game if the colours are presented too often; moreover it was necessary that the last experiments should not be separated from the first by too long an interval if the results were to be added together, for rapid changes and development are taking place in the mind of the child at this early period¹.

At each sitting those colours were selected for use which had been used least often previously, so that any effects due to novelty were reduced to a minimum. For this reason also the same colour was never used twice on the same day. Occasionally two sittings were taken on the same day, usually one being early in the day and the other towards the end. If the baby appeared restless or discontented, the sitting was abandoned.

A very important question was *the relative brightness of the colours used*. Owing to the generally acknowledged attraction of brightness for infants it is obviously unsafe to draw inferences as to *colour* preferences if the colours used are of unequal brightness. It does not seem satisfactory for the experimenter to rely upon the general immediate impression of brightness produced by the colours. Three different tests were therefore used to determine the relative brightness of the colours (see Appendix). The results of the three tests were in substantial agreement, and indicated that the yellow and pink were equally bright and equivalent in brightness to a grey of 185° W. + 175° Bk. The green, blue, violet, red and brown were also practically equal in brightness, and equivalent to a grey of 32° W. + 328° Bk.

¹ I regret especially, however, the omission of grey. At first I had in mind solely the question of colour preferences. It was only after I had performed a fairly long series of preliminary experiments that I became interested in the question of colour sense development, and then results obtained with a new wool suddenly introduced would have been unreliable. Otherwise it would have been well to have included a dull grey equivalent to the red, green, blue, and violet in brightness and a bright grey equivalent to the yellow in brightness, dropping perhaps the pink, and black or brown.

IV. *Results of Experiments.*

Table I shows the number of gains for each colour. The sign + when placed horizontally opposite to any name indicates that this colour proved superior to the colour named at the top of the vertical column in which the sign stands; similarly the sign - indicates that that colour proved inferior in attractiveness; while the sign ? indicates that the scores were so small or so nearly alike that the experiment is regarded as indecisive.

TABLE I.

	Yellow	White	Pink	Red	Brown	Black	Green	Blue	Violet
Yellow.....		+	+	+	+	+	+	+	+
White.....	-		-	+	+	+	+	+	+
Pink.....		+		+	+	-	+	+	+
Red.....	-	-	-		+	+	-	+	+
Brown.....	-	-	-	-		+	?	+	+
Black.....	-	-	+	-	-		?	+	?
Green.....	-	-	-	+	?	?		-	+
Blue.....	-	-	-	-	-	-	+		?
Violet.....	-	-	-	-	-	?	-	?	

This gives us the following order :

TABLE II.

	Gains	Losses	Balance of gains over losses
Yellow	8	0	8
White	6	2	4
Pink	6	2	4
Red	4	4	0
Brown	3	4	-1
Black	2	4	-2
Green	2	4	-2
Blue	1	6	-5
Violet	0	6	-6

In these tables, however, as much value is given to a gain by only a small majority as to a gain by an overwhelming majority. It is important therefore to compare the above order with that given in Table III. Here all the scores (in seconds) of each colour have been added up, together with the scores against them.

It would be unfair to draw up an order merely upon the total scores of each colour. For such conditions as the child's mood, or the light, may have been exceptionally favourable on those days when some particular colour, say pink, was used, and exceptionally unfavourable on

the days when some other colour was used. Thus green in Table III has 165 to its credit—about the same as brown (151). But far more seconds were scored *against* green (421) than against brown (275). We therefore get a fairer basis of comparison by adding the score of green (165) to the score against green (421), which gives us the total scored during the experiments in which green was used, viz. 586 seconds: and then finding what *percentage* of this total was scored by green. Such percentages are also given in Table III.

TABLE III.
Showing the total scores for each experiment.

	Yellow	White	Pink	Red	Brown	Black	Green	Blue	Violet	Totals A		Totals B		Percentage scores	
										For	Against	For	Against	A	B
Yellow		76	17	106	31	88	133	50	24	525	137	282	48	79·3	85·4
White	42		48	70	104	53	153	22	188	680	246	487	108	73·4	81·8
Pink	4	96		46	79	19	77	102	53	476	186	289	68	72·2	80·9
Red	23	24	26		40	45	6	39	39	242	283	75	153	45·3	32·8
Brown	8	3	0	28		33	4	37	38	151	275	65	276	37·8	19·0
Black	0	38	39	10	18		3	37	4	149	263	152	109	35·7	58·2
Green	23	9	19	20	0	0		0	94	165	421	54	276	28·2	16·3
Blue	37	0	37	3	3	13	22		7	122	300	18	106	28·9	14·5
Violet	0	0	0	0	0	12	23	13		48	447	9	287	9·7	3·0
Totals	137	246	186	283	275	263	421	300	447						

In column A all scores of 3" and over are reckoned. In column B only scores of 8" and over are reckoned. That is, when W. only looked at a colour 7" or less continuously, that particular score was ignored.

Table III (percentage scores A) gives us an order almost identical with that of Table I, the only change from Table II being that green and blue are now brought almost on a level with one another.

Order based on Table III.

yellow	79·3%	black	35·7%
white	73·4	blue	28·9
pink	72·2	green	28·2
red	45·3	violet	9·7
brown	37·8		

Consistency Test. By comparing Tables I and III we can obtain some indication as to consistency. For example, judging from Table III, brown ought to be the winner in the experiments in which it was compared with black, blue, green and violet, and the loser when compared

with yellow, white, pink and red. On inspecting Table I we find that all these results were obtained, except that the experiment with brown and green was regarded as inconclusive (brown 4—green 0). Hence the consistency score may be reckoned as at least $7\frac{1}{2}$ out of 8.

Proceeding in this way we find the following consistency scores: yellow $\frac{8}{8}$, white $\frac{7}{8}$, pink $\frac{6}{8}$, red $\frac{7}{8}$, brown $\frac{7\frac{1}{2}}{8}$, black $\frac{6}{8}$, blue $\frac{7\frac{1}{2}}{8}$, green $\frac{6}{8}$, violet $\frac{7}{8}$, a total of 31 cases (out of 36) in which the results of individual experiments were consistent with the order given by the total scores. If indecisive experiments are ignored the result is only three inconsistent results in 32. When one considers the possibility of changes of interest and mood from day to day this degree of constancy seems very satisfactory.

V. Discussion of Results.

Two kinds of inferences can be made from such results as we have obtained—(1) inferences as to colour preferences, (2) inferences as to the development of the colour sense. We cannot of course infer from the *absence* of preference between two colours the absence of any difference of sensation¹. But from evidence showing that one colour is markedly preferred to another, we can infer that the colours are sensed as different colours, unless the preference can be ascribed to differences of brightness.

(u) *The Presence of Colour Sensations.* W. is obviously attracted by the brightness of objects. White comes second in the list, and yellow and pink, by far the brightest of the colours, are first and third respectively.

But there is, I believe, adequate evidence that brightness is not the *sole* cause of some of the coloured wools being preferred to others. The strongest evidence of this is to be found in the great difference between the score for violet (9·7 %), on the one hand, and the scores for the other colours of equal brightness, especially red (45·3 %) and brown (37·8 %). Further evidence is afforded in that the score for red is higher than the scores for blue and green, although they are each as bright as the red.

Among the warmer colours, then, red and brown must owe their high position to the fact that their colour was both sensed and liked. Green and blue are almost equal on the scale. But both of them were

¹ Cf. C. S. Myers, *op. cit.* 358. McDougall himself explicitly states the logic of the question on page 346 of his article.

distinctly preferred to violet, so that sensations of blue and green were presumably also experienced¹.

The fact that yellow is more attractive than the equally bright pink and even more attractive than white suggests that also yellow is both sensed and liked. The difference between the percentage scores of white and yellow is small, it is true; but even if yellow had only scored the same as white one might reasonably suppose that the colour of yellow was appreciated. Otherwise, if the yellow wool appeared only as a bright grey, one would expect a superior score for white owing to its superior brightness. It may be suggested that a bright grey might be preferred to a pure white owing to the latter being too glaring in a strong light, and that this might account for the yellow, even if only sensed as grey, being preferred to white. But evidence against this is found in the fact that white actually scored *more* on the bright days than on the dull days, the totals being as follows:

Score of white	On four bright days	On four dull days
			352 secs.	328 secs.

Furthermore, as has so often been observed in the case of infants of this age, W. showed a delight in looking directly at very brightly lit windows and at incandescent lights.

In any case we have seen that W. was almost certainly sensitive to red, brown, and probably blue and green as such, and according to every important theory of colour vision it is most unlikely that these colour sensations would be developed in the absence of yellow.

There is no appreciable difference between the scores for blue and green, and consequently no direct proof of discrimination. But the difference between the scores for blue and green on the one hand and for violet on the other indicate that some colour was perceived in the blue and green wools.

That blue and green should have similar affective values for an infant is not surprising in view of the fact that such seems to be the case also with much older children; at least blue and green are frequently confused by them. It has however been shown by Miss A. W. Tucker² that the weakness of young school children in the discrimination of blue

¹ Unless one can attribute the different positions of these colours to the fact that the colours themselves are sensed, one must introduce some hypothesis to the effect that the colours have brightness-values for the eye of the infant which are different from their brightness-values for the eyes of adults. This has never been disproved, but it seems highly improbable that the great differences between the scores of red and brown (for example) and that of violet are to be so explained.

² This *Journal*, iv. 33.

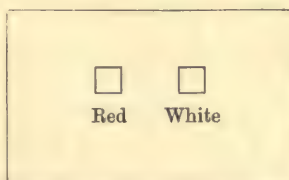
and green cannot be traced to lack of sensitivity to either blue or green, but must be referred probably to psychological causes. A similarity in the affective values of the two colours would doubtless aid in such confusion.

Summing up the results gained so far we may conclude that probably the sensations of red, yellow, green, blue and brown may all be experienced by a child about three months old¹.

There remains the case of *violet* to be considered. These experiments afford no direct proof that violet was sensed. One can only fall back on the argument already referred to, namely, the difficulty of explaining, on any theory of colour vision which receives wide support at the present day, the existence of the sensations red, yellow, green and blue, if violet is only seen as grey. If the physiological mechanism required for red and also that for blue are functioning already it is difficult to see why violet should not also be sensed.

(b) *Colour Preferences.* The order of colours as given in Table III calls for some comment. McDougall suggests² that the relative attractiveness of different colours for a child may be determined by two factors, namely, brightness and novelty. We have already remarked upon the influence of brightness, revealed by the high position of yellow and pink as well as of white. That W. was also susceptible to the influence of novelty, as were the children of McDougall and Myers, was shown by the following short series of tests carried out shortly after the experiment just described.

I procured some paper of a red colour, considerably brighter than the red Holmgren wool, and slightly tinged with orange. A piece of cardboard, 10 cm. square, was covered with this paper, and was affixed to a large wooden board covered with dark grey cloth. A piece of white cardboard of the same size as the red square was also affixed to the board, about one foot away from the red square. The board would appear thus:



¹ To speak more strictly we should perhaps say that red, yellow, green stimuli give rise to colour sensations in an infant of that age. We cannot of course prove that the sensations experienced are exactly similar to those which are caused by these various lights in adults.

² *Op. cit.* 349.

This board was held on my knees in front of W. for $1\frac{1}{2}$ minutes, the cardboard squares being equidistant from his central line of vision, and, as before, we noted the length of time he looked at either of the cards. The positions of the cards were then reversed (the previous right hand card being now on the left and *vice versa*), and after an interval the board was held before the child for another $1\frac{1}{2}$ minutes.

Table IV shows the respective scores of red and white on successive days.

TABLE IV.

			Red	White
1st day (dull) ¹	121 secs.	12 secs.
2nd day (bright)	81 "	33 "
3rd day (dull)	78 "	10 "
4th day (dull)	6 "	13 "
5th day (moderate)	18 "	17 "
6th day (bright)	3 "	3 "

These figures show most strikingly the effect of the loss of novelty upon the attractiveness of the red square.

At the same time, I do not think that the order of relative attractiveness of the coloured wools for W. can be explained merely by the two factors of brightness and novelty. We have already seen that the order of the colours red, brown, green, blue and violet cannot be explained by reference to their degrees of brightness, as they were all of equal brightness. As regards novelty, violet was certainly the most novel colour for W.; yet it was liked least of all. On the other hand yellow, his favourite colour, was the colour of his nursery wall-paper, while his favourite plaything and a constant companion in his cot at this time was a pink eiderdown quilt. Again, neither blue nor green would be very familiar to him, for unlike McDougall's child, he had seen practically no foliage, the experiments taking place in the spring in Scotland. Black he was very familiar with; I had previously noted how often he was to be found gazing at a black piano, black hats, black boots, etc. Yet black is liked better than the less familiar blue, green and violet.

The novelty that attracts, in such experiments as these, may, it seems to me, be the novelty of the specific object with its specific colour, rather than the rarity of the colour in the ordinary everyday experience of the child. If a new wool is introduced suddenly into the middle of a long series of experiments, I am inclined to think that its novelty would attract for a few days, however familiar the colour might be to the child. Where, as was the case in the experiments with W.,

¹ The references in brackets are to the light on each particular day.

the presentations of the various colours were equally distributed over the whole series, this particular source of novelty-effect would be reduced to a minimum.

I would suggest in the most tentative manner, that at the early age of three months, the relative attractiveness of any or all of the colours is determined by a factor more fundamental than novelty, namely, their comparative powers as stimuli to the organism.

Myers has already urged that the attractiveness of red for infants and savages is of a very fundamental nature, and he refers to the "excitatory action" of red upon organisms lower than man¹. Féré found that the general stimulating effect of colour is greater in the case of colours at the 'warm' end of the spectrum than of those at the violet end. The strength of his subjects, as tested by the handgrip, was at its highest when the patient was under the influence of red light, at its next highest with orange and yellow, less with green and blue, and least with violet².

Now if it be supposed that yellow and pink owe their high position partly to their brightness, then the order of the colours as preferred by W. is remarkably like that given by Féré. It may of course have been accidental that W. happened to like the colours in the order given, and it would doubtless be unwise to press this suggestion as to the dependence of the order of preference upon the powers of stimulation possessed by the colours, unless similar results are obtained from experiments upon other infants at a similar early age.

Several observers, as we have seen, agree upon a preference for 'warm' colours among infants of from six to twelve months old. But I am not aware that anyone has hitherto observed such a marked indifference (or possibly aversion) to violet as was shown by W.³, or such a preference for brown and even for black before the spectral neighbours of violet, viz. blue and green.

It is possible that such extreme indifference to violet, blue and

¹ *Op. cit.* 362.

² See Féré, *Sensation et Mouvement*, Paris, 1900, Chap. vi.

³ The records show that violet was looked at almost as often as green, blue, brown and even white, so that its low score cannot be attributed to its escaping notice. The figures for all colours were: violet 62, green 70, blue 72, brown 73, white 75, red 87, black 100, yellow 110, pink 114. In making this calculation each glance given by W. to a wool is scored as one, whether he looked at it for a minute or only for one second. Evidently violet did not suffer through lack of opportunity. These figures indeed suggest that the position of a colour in the order of preferences depended very little upon its power of causing a reflex movement of the eyes towards it; see especially the low score of white in this list, less even than those of red and black.

green, even if found in many children, may not be characteristic of them beyond the age of three or four months. (W., as we shall see, had changed considerably by the age of seven months.) It is even possible that young infants under, say, four or six months may resemble in this respect the hysterical subjects of Féré, who proved to be more sensitive to the different stimulating effects of the various colours than were normal subjects.

But the preference for *red* continues beyond the period of infancy. Thus experiments on children in Antwerp¹ showed that red was the most pleasing colour from four to nine years of age; and Winch, experimenting in London schools, found that red was the best-liked colour among Standard I children, yellow coming generally second or third².

VI. *Experiments by the 'Grasping Method.'*

When W. was seven months old I began further colour experiments by means of the grasping method. Experiments were performed practically every day for a month. The coloured objects used were the same wools as were used in the previous experiments, except that brown was omitted and a grey of the same brightness as the red, blue, green and violet was introduced. Also I used a white wool instead of white linen.

Two of the wools were placed on a table covered with a dark grey cloth. W. was comfortably seated on his mother's knee, and one wool was placed opposite each shoulder, about eight inches from the edge of the table, just out of the baby's reach. While the wools were being placed in position they were hidden by a black screen. About three seconds after this screen was removed, W. was brought slightly forward, bringing him close up to the table and within reach of the wools. This plan was followed because at first he was so eager to grasp *anything* within reach (including the screen) that he frequently seemed to seize one colour without having looked even momentarily at the other, so that there was no real 'choice.' In spite of these few seconds' pause, my impression still was that W. was often so eager to grasp a wool, that the specific colour of the wool was comparatively unimportant, and that only a very strong preference for a colour would cause it to be chosen more often than its partner.

¹ By Schuyten, quoted by Meumann in his *Experimentelle Pädagogik*, zweite Auflage, 1911, 244.

² This *Journal*, III. 42.

A note was taken as to which wool was seized, and as to which hand was used. When W. grasped both colours simultaneously, each colour scored a half-mark. The same wools were then arranged in a position the reverse of the preceding, *i.e.* the wool that had been on W.'s right was now on his left and *vice versa*. The same pair of wools was presented to W. ten times at one sitting, the position being reversed each time. On the next day two other colours were similarly presented ten times, and so on, until each of the wools had been presented with each of the others. Great care was taken, each day, to choose those colours which had remained unused for the longest period, thus minimising any effects due to novelty. Table V gives the results of these 36 experiments involving 360 choices.

TABLE V.

	Yellow	Red	Pink	Grey	Violet	White	Green	Black	Blue	Total
Yellow		5½	5	5	8	7	6	7½	7	51
Red	4½		6	5	7	6½	6	5½	5	45½
Pink	5	4		5½	5	7	7	6	5	44½
Grey	5	5	4½		5	5	5½	6	5	41
Violet	2	3	5	5		5	5	5	7	37
White	3	3½	3	5	5		5	5	7	36½
Green	4	4	3	4½	5	5		5	5	35½
Black	2½	4½	4	4	5	5	5		5	35
Blue	3	5	5	5	3	3	5	5		34

It will be seen that, as at three months, yellow still holds first place. Red and pink are now almost bracketed second.

The most striking difference apparent between the results of the experiments at seven months and those at three months is the drop in the comparative attractiveness of brightness. White is now only on a level with violet, green, blue and black, and at least not more attractive than a dull grey¹. Pink, also, scores only about the same as red, while at three months it was much preferred to red.

It should however be made clear that W.'s interest in the colours during these later experiments seemed to be slight compared with his enormous interest in the game of merely grasping them. Moreover,

¹ It is conceivable, though it seems to me very unlikely, that the prolonged series of experiments with these same wools from the age of 3 months to that of 4 months may have resulted in these colours being felt vaguely as familiar even three months later in this second series of experiments. In which case the comparatively high position of grey (compared *e.g.* with blue) may be due partly to its novelty, as grey was not used in the previous experiments.

the scores of the various colours were levelled to some extent by the habit of using always the same hand. At first W. used almost exclusively the *right* hand, choosing generally the colour on the right. I attempted to get over this difficulty as Myers did, by placing the wools in the median plane, one being about four inches nearer the child than the other¹. But I found that W. invariably took the colour nearer to him, so this method was abandoned and the results gained by it were not counted. Another variation of arrangement was attempted. One wool was placed about three inches to the right and the other about three inches to the left of a point immediately in front of W.'s right shoulder, so that each wool was about equally well placed for grasping with the right hand. Strange to say, this method had not been in use for more than two sittings (experiments nos. 14 and 15) when suddenly, on the occasion of the 16th sitting, W. began to use his *left* hand more than the right. In spite of the unfavourable position of the wools for the left hand, the left hand was used seven times and the right only three times at this sitting. I therefore at once reverted to the original plan of placing one wool immediately opposite to each shoulder, and this arrangement was used throughout the remaining experiments. The new preference for the use of the left hand continued to the end of the experiments.

It seems extremely likely that a habit of using one hand predominantly may level the scores of the various colours to an extent which conceals the real preferences of the child for certain of the colours, though of course one can at least infer that these preferences are not strong enough to overcome the tendency to use one hand more than the other. Occasionally, in the midst of a long run chiefly of left-hand grasps during the latter half of the experiments, the liking for a certain colour would apparently assert itself and the right hand would be used in order that this particular wool might be taken; *e.g.* in experiment no. 24 (white and red) in ten grasps the right hand was used five times alone and three times simultaneously with the left, but always in order to grasp the red.

Occasionally, too, the left (or right) hand would cross over to seize the wool on W.'s right (or left). The numbers of times the various colours were seized in this way were as follows:

yellow	8	white	3
red	5	black	3
pink	4	green	3
grey	4	violet	3
blue	4		

¹ *Op. cit.* 357.

However, in spite of these possible variations in the way of grasping, my impression was that, with so active a child as W., the eagerness to grasp *anything* must materially level the scores of the various colours. Furthermore, as this eagerness to grasp might be exceptionally strong some days whilst on other days there might be a greater tendency to attend to the colours as such, I would suggest that the method is far from being so entirely free from all objection as has been thought. Thus if W. were fatigued or bored with colours on a day when yellow and grey were presented he might very well every time grasp with his then favourite hand (left); thus the score would be yellow 5, grey 5 (as was the case). He may also have been in a mood comparatively indifferent to colours when he chose grey five times and violet five times. These figures would suggest that the yellow and violet were equally pleasing to him. Yet on another day, when more alive to the influence of colours, he might choose (as he did) yellow eight times and violet only twice, which suggests that the previous tests had been unduly favourable to violet or unfair to yellow.

In view of this difficulty it seems to me that, when the subject is so eager to grasp as W. was and falls so readily into a habit of using the right (or left) hand predominantly, the grasping method is not so satisfactory as the method used with W. at 3 months, if the precaution is taken to reckon the percentage scores (see page 370) when using the latter method. It is evident, however, that the difficulty mentioned does not apply equally in the case of all children. McDougall's child, for example, "grasped with both hands in turn usually with free alternation, the use of the right hand predominating a little, although she is by nature left-handed¹."

The only important point in which my results fail to agree with those obtained by McDougall in his long series of experiments with L. is the preference of L. for blue, green and white before grey. This result I should be inclined to take as more reliable than the results given by W., owing to the difference between the children which I have just mentioned.

Of course one must guard against the assumption that the same results ought to be given by different children. Individual differences may exist between infants of six months in reference to colour preferences, as they do in reference to numerous other activities in the life of a baby².

¹ *Op. cit.* 343.

² Further, completely satisfactory comparisons are not possible owing to our ignorance of the comparative brightness and saturation of the colours used by different observers.

Not only may colour preferences vary among different infants, but the attractiveness of colour in general is also doubtless greater with some than with others. In the case of W. the desire to look at colours seemed to be much less at seven months than at three, owing no doubt to its being ousted by the exceedingly strong desire to handle anything and everything. When W. was eight months old I made one trial of the method used at three months, but was soon convinced that he would not look for more than two or three seconds at a time at any colour, so eager was he to be doing something.

VII. *Experiments by the method of Grasp and Reward.*

At the end of the experiments just recorded, when W. was 8½ months old, I attempted a further series by means of the method of 'grasp and reward' suggested by Myers¹. No very definite results were obtained, but I give some account of this set of experiments as an example of this new method.

So far there had been no proof of the discrimination of blue from green by W., though we had seen reason to believe that even at three months they were each discriminated from violet and red. I therefore presented the green and blue wools, as in the grasping experiments just described, but with the following addition. Whenever W. took the blue wool he was at once rewarded with a sip of honey or jam. At first the pairs of wools were presented twenty times at each sitting.

The association between reward and grasping (something) was soon established. At the second sitting, I recorded "when W. has taken green and gets no reward he is very eager to seize the blue²." At the fourth sitting I recorded "mouth open for jam as soon as wool (blue) taken," and again "blue taken very deliberately. W. looked round at once for jam" (the jam being on a plate on my right and W. on my knee). Thenceforward W. very frequently looked round for jam, but sometimes when green was taken.

It soon became evident that he tired of the honey or jam (though only allowed a very tiny sip) before the end of the twenty presentations. Thus at the seventh sitting, during the first ten presentations W. turned every time to his right and evidently enjoyed the honey, whereas in the second ten presentations he only turned once (when blue was

¹ *Op. cit.* 354.

² Previously he had almost invariably been content to play with the one colour seized. This attempt to seize the second wool *may* have been only a development of the tendency to seize both wools, which McDougall observed as his children grew older.

chosen). Thereafter I never gave more than ten or twelve presentations at a sitting.

Results gained by the Method of Grasp and Reward. It seemed at first a reasonable plan to divide the twenty choices of each sitting into two sets of ten. If any association were built up in the course of the sitting between blue and 'reward,' blue would tend to be chosen more frequently in the second half than in the first.

Taking thus the seven sittings, of twenty choices each, we get the following figures:

Number of times chosen	First half of each of the sittings		Second half of each of the sittings	
	Blue	Green	Blue	Green
	33	37	37	33

The increase in the number of times blue was chosen is seen to be inappreciable. But probably it is unsatisfactory to divide the figures thus. For as has already been pointed out, twenty choices proved too many at a sitting, the desire for the reward obviously slackened and sometimes seemed to change to aversion. More reliable results are probably obtained by considering only the first ten choices. Comparing the results given by only the first five choices and the second five choices in all the eight sittings¹, one gets the figures

Number of times chosen	First five choices of each sitting		Second five choices of each sitting	
	Blue	Green	Blue	Green
	14	26	21	19

Here there is a distinct suggestion that the association is being established; green scores nearly twice as many as blue in the total based on the first five choices of each sitting, but barely as many as blue in the total based on the second five choices of each of the sittings. The numbers are too small perhaps on which to base any confident assertion.

My own general impression, if that counts for anything, was that the association seemed to be made *fleetingly*; during some sittings it would seem even strong, and then again it would entirely vanish. If there were any association it certainly was of this fleeting and variable nature, though of course that is just what one would expect, judging from the beginnings of association in general in the child mind. Certainly also it was not carried on from day to day. For during the

¹ One sitting of 10 choices was added after the seven sittings of 20 choices.

first four sittings blue was chosen twenty times and green twenty, while during the last four sittings the numbers were, blue fifteen, green twenty-five times.

I also tried the grasp and reward method with red and blue. I argued that if a much stronger association between blue and reward were built up when blue had only to be distinguished from red, the absence or weakness of association in the blue and green tests would suggest that blue and green were not discriminated so well as blue and red. On the other hand if no stronger associations were built up when red and blue were used, the failure to establish an association when green and blue were used need not be ascribed to lack of discrimination between blue and green, but would show that at this age an association could not be formed between one of two definite colours and a reward, even when (as in the case of red and blue) the colours certainly gave rise to different sensations.

In the red-blue experiments W. was rewarded when he chose blue, as in the blue-green experiments¹. The results were as follows:

		First half of each sitting ²		Second half of each sitting	
		Blue	Red	Blue	Red
Number of times chosen	18	25	22	21

These results show less evidence of an association between blue and 'reward' than was the case in the blue-green experiments. Nor is there any indication that the association was established gradually from day to day. For, in the course of the first four sittings, blue was chosen nineteen times and red twenty-five times; during the last four sittings the figures were, blue twenty-one, red twenty-three times.

In so far as these experiments with red and blue show *less* evidence of the association than did the green-blue experiments, I think it may be ascribed partly to the cause already mentioned (in the first footnote to this page), and partly to the growing indifference of W. to the rewards, though these were varied from time to time. I attempted still further tests with blue and green after the red-blue series but abandoned them

¹ Thus the 'reward' had to contend with the natural preference for red. No doubt this hindered to some extent the formation of an association between blue and reward. (Cf. Miss E. M. Smith's findings with dogs, "Some Observations concerning Colour Vision in Dogs," *This Journal*, v. 176.) Obviously however no satisfactory conclusion could have been made if the choice of the already preferred red had been rewarded.

² Each sitting in the Red-Blue series comprised 10 or 12 choices. As red was preferred to blue by W. we should expect a larger number of choices of red until some association between blue and reward was established.

In the later series of 'grasp and reward' experiments immediately following, W. was still more addicted to the use of the left hand, the figures for the last 120 grasps being: Right hand 15, Left hand 105.

It is interesting to note that this specialisation of the left hand for the purpose of wool grasping was not accompanied by any left-handedness in other activities. W. still seemed to use his right hand more than his left in his play, particularly in dealing with heavy objects, such as books. McDougall also remarked that his child L. was by nature left-handed, yet used the right hand somewhat more frequently in the grasping experiments. During the early months of W.'s life, his right-hand reflex grasp was stronger than his left; and when voluntary grasping began, the right hand learned to grasp much more readily than the left. In the sixth month he was often seated on my knee by the open piano, and had learned to hit the notes, greatly enjoying the sounds, or his own production of them. This performance, which was done at first with right or left hand indifferently, was soon relegated largely to the *right* hand, and during the eighth month (in which the specialisation of the *left* hand for wool grasping was observed) the right hand was used almost exclusively by W. when thumping the piano.

Thus there appears to be specialisation of the right hand in one action, and of the left in another action even at this early age. If this were shown to be frequent in infants it might throw a new light upon the problem of right- and left-handedness. Possibly in the case of a right-handed child, the left hand may tend to specialise in actions which are very simple, thus setting free the right hand for more serious work.

Baldwin found that more distant objects called forth the right hand in a right-handed child, but that objects at an easy distance called forth either hand indifferently. Attractive colours, he believes, act like objects at a greater distance.

Perhaps, then, one may explain the change observed in W. from the predominant use of the right hand to the use of the left somewhat as follows. At first, the coloured wools, by their strong attractiveness drew out the right hand of W., naturally right-handed. As, with practice, grasping the colours became easier, and as with familiarity the wools became somewhat less attractive, the right hand was called forth less, and the easy task of grasping relegated largely to the left hand. But if this is a correct surmise it remains something of a mystery why the change from right hand to left should begin just during those experiments in which the wools were most favourably placed for the *right* hand.

IX. SUMMARY OF RESULTS AND CONCLUSIONS.

I. There is good evidence that at the age of three months an infant may experience the sensations of red, yellow, brown, green and blue.

II. In the case of W. the order of preference of the colours used was as follows: yellow, {white}, {pink}, red, {brown}, {black}, {blue}, {green}, violet.

III. The order of preference seems to be partly determined by brightness, but cannot be explained entirely by reference to brightness or to novelty.

IV. It is suggested that the order of preference is partly determined by the relative powers of the various colours as stimuli to the organism.

V. At seven months the same infant still liked yellow best of all the colours used, and then red and pink. By this time the comparative attractiveness of white had decreased, being no greater than that of violet or even grey.

VI. There was some suggestion of an association between the grasping of the blue wool and the idea of a reward, when blue and green wools were offered to W. The lack of more definite association can be ascribed to the difficulty of establishing *any* association of such a nature at this age, and need not be attributed to failure to discriminate blue and green.

VII. In the course of the grasping experiments W. developed a habit of using the left hand almost entirely in spite of the fact that he showed a distinct tendency to right-handedness in his ordinary actions.

APPENDIX.

Determinations of the brightness values of the colours used.

Test 1. The colours were placed, two at a time, on a dark grey background, in the extreme periphery of the field of vision of an observer, who fixated a point immediately in front of him. One colour was placed some eight inches above the other and the observer was asked to say whether the upper or lower colour appeared the brighter. By this method the yellow and pink, naturally the brightest of the colours, were judged to be about equally bright, and of the other colours, the violet appeared slightly brighter than the blue

and green, which in their turn were very slightly brighter than the brown and red. This method, however, was not easy to carry out satisfactorily owing to the persistence of the appearance of blue colour in the blue and violet wools. But the very difficulty found in saying which of the colours was the brighter, which always occurred with any pair of the darker group (green, blue, violet, red and brown), is itself an indication of the close resemblance of these colours as regards their degree of brightness when seen in the peripheral field of vision.

Test 2. Each of the colours was placed in turn in the peripheral field upon various shades of grey, until a grey was discovered of the same degree of brightness on which the shape of the wool vanished. Two subjects were tested. The yellow and pink were judged equal in brightness to the same grey paper (equivalent to 185° W. + 175° Bk.). The violet, blue, green, brown and red were also judged approximately equal in brightness to another grey paper (equivalent to 32° W. + 328° Bk.). The blue and green appeared perhaps slightly brighter than the others.

Test 3. Lastly the *flicker test* was used. A long piece of one of the wools was closely gummed on to a piece of cardboard, the wool being placed in parallel lines so that no interstices were left¹. Similar cards were made with all the wools.

Two subjects were tested by means of the flicker test. Each of them found that the flicker disappeared simultaneously in the case of the pink and yellow wools. Each member of the darker group of colours (green, blue, red, brown and violet) was tested with each of the other members of the group. In every pair of colours flicker disappeared practically simultaneously. Thus the brightness of the colours as seen by direct vision was approximately the same. The equivalence of the brightness values of the various colours and those of the greys, the black-white values of which have been given above, was also confirmed by the flicker test.

¹ This method was suggested to me by Dr C. S. Myers. See his *Text Book of Experimental Psychology*, Part II. Experiment 68, for details of method.

(Manuscript received 6 November, 1913.)

THE TESTIMONY OF NORMAL AND MENTALLY DEFECTIVE CHILDREN¹.

BY T. H. PEAR AND STANLEY WYATT.

(From the Psychological Laboratory, University of Manchester.)

- I. *Introductory.*
- II. *Description of the experiments.*
- III. *Subjects taking part in the experiments.*
- IV. *The children's testimony.*
- V. *Treatment of the results.*
- VI. *The results obtained.*
 - A. *The 'narrative.'*
 - B. *The 'interrogatory.'*

The categories in detail; items, colours, sizes, duration, sequence.

'Reconstruction' of the event.
- VII. *Conclusions.*

I. INTRODUCTORY.

Two important questions in connexion with the psychology of children's testimony still remain unanswered. They are²:

(1) How far is the testimony of normal children reliable for ordinary purposes?

(2) To what degree, and in what directions, does the testimony of the mentally defective child differ from that of the normal child of the same physical age?

Examples of the disagreement in the answers to these questions may be given here. Babinsky³ declares that children are the most dangerous

¹ Amplified from two papers read before the Sub-section of Psychology at the Meeting of the British Association for the Advancement of Science, Birmingham, September, 1913.

² The questions have been formulated by Whipple, "The Psychology of Testimony," *Psychol. Bull.* 1911, viii. 307.

³ *Die Kinderaussage vor Gericht*, Berlin, 1910.

of all witnesses, and demands that their testimony be excluded from court record wherever possible. Similar statements are made by Duprée¹. Gross², however, stakes his thirty years' experience in the court against the views of these physicians. He declares that a healthy half-grown boy is the best possible witness for simple events³.

With regard to the testimony of mental defectives, little experimental investigation has been undertaken⁴, and opinions seem limited to the statement that their testimony is less valuable than that of normal persons, or to similar expressions which give us no information of psychological value.

The present experiments, by comparing the testimony, of the same event, given by normal and by mentally defective children, attempt to obtain some facts which may throw light on the two questions mentioned above. They deal, however, only with children of school age, so that the related question of the comparison of the testimony of normal and of mentally defective *adults* is still untouched. Many other points towards the elucidation of which the experimental results offer some evidence will be treated later. Some of the chief are: 1. The comparative value of the testimony, of the *same* event, of normal and defective children. (Very often the comparison has not been made in this way, but *different* experiments on the two classes of subjects have formed the basis of the statements made.) 2. The relative value of 'spontaneous' and 'interrogated' evidence. 3. The relative effects of different kinds of suggestive questions upon the two classes of children. 4. The effect of repeating the testimony after a long time, in which the subjects have had the opportunity to think over the event and to discuss it with their friends, including both those who have, and those who have not witnessed it.

It may be pointed out that in several respects the present experiments differ from those which have been performed elsewhere. In the first place, there seems to be no published record of any experiments on

¹ "Le témoignage: étude psychologique et médico-legale," *Rev. d. deux Mondes*, 1910, LV. 343-370.

² "Zur Frage der Zeugenaussage," *H. Gross' Archiv*, 1910, xxxvi. 372-382 (cited by Whipple, *op. cit.*).

³ Whipple, *op. cit.* p. 308.

⁴ For a general account of the experimental work on the psychology of testimony see Whipple, *Manual of Mental and Physical Tests*, Baltimore, 1910 (gives bibliography up to 1909); *Psychol. Bull.* 1910, VII. 2; 1911, VIII. 9; 1912, IX. 7; 1913, X. 7; Duprée, *op. cit.* The most important sources are Stern's *Beitr. z. Psychol. d. Aussage*, Leipzig, 1903-6; *Erinnerung, Aussage und Lüge*, Leipzig, 1909, and articles in the *Ztsch. f. angew. Psychol.*, especially 1911, IV. 378-381 (bibliography from 1908-10).

testimony in this country; most of the work having been done on the Continent or in America. It is possible that the spontaneous interests of children in different countries are not identical: so that one cannot conclude that experiments performed *e.g.* in Germany give results which are valid for England. Comparative work of this kind is needed. Again, the event used as the basis of these experiments was, we believe, more complex and richer in incident than those previously used. It was also repeated six times before subjects of different grades of mental efficiency, and in different environmental conditions. Finally, the number of subjects (143) was larger than usual.

II. DESCRIPTION OF THE EXPERIMENTS.

The 'picture test' and the 'event test'; their relative advantages.

Previous workers in this field have employed, as the object on which a report has to be given, either a picture or a pre-arranged event. Both forms of test have their own advantages and disadvantages. The picture can be made very complex; it may contain a great number of items, colours, positions, etc.: it is a constant and invariable stimulus, and so can be employed many times on different classes of subjects, for purposes of comparison. Its disadvantages are equally obvious. It lacks solidity, movement, and temporal sequence—three facts which detract greatly from its value in an experiment which is intended to approach the natural conditions of ordinary life. Again, it is exceedingly difficult to present a picture to a class of subjects for a definite time, and still to keep them in ignorance that they will be required to report afterwards upon what they have seen. The 'event' as a test, when carefully arranged, suffers from none of these disadvantages, but up to the present, it seems to have been comparatively simple, so that there has been relatively little to report when it was over, and hence the opportunity of studying individual differences has not been very great. The repetition of the event, too, in front of other classes of subjects, does not seem to have been carried out very often, perhaps because of the difficulties inherent in such a procedure.

The present experiments attempted to combine the richness in detail and complexity of the 'picture test' with the naturalness of the 'event test.' One of the items figuring in the event was a picture, so that we have here one class of test inside the other. The test, too, was repeated six times before different groups of subjects. The repetition

of the test made it possible to use small classes of subjects at any one time, so that they all could easily see what was happening.

The event chosen. Requirements to be fulfilled. It was desirable to select an event which, although containing more incident than those used by previous workers, was still capable of exact repetition. It must also be striking, but not artificial in appearance, in order to avoid the arousal of the children's suspicion. Again, the event must not be too striking, or an unusual amount of subsequent reflexion upon it would have been provoked.

Since items which are connected with living persons prove to be of great interest in such circumstances, two persons of different sex were selected to take part in the event, and moreover, since, under ordinary conditions, an event does not always show only one well-marked focus of interest, both persons were active at the same time. The fact that both in the 'performers' and in the onlookers in the event the two sexes were represented makes it possible to ascertain the direction of the interests of the various subjects in the performers.

It may seem that the event to be described, complex and full of interest as it was, would have been likely to arouse the suspicion in the children that some kind of test was being carried out. To find whether this was the case, at every school the children were asked, after they had given their testimony, if they had suspected anything of the kind, or if they had thought that the event was anything but an ordinary visit to the school. One child only, out of the 143 subjects, had had any suspicion, and he "thought that it might be a memory test." (This was at the Fielden Demonstration School, Manchester.) But the boy was careful to add that on that account he did not tell any of the others what he had thought. In all cases the children were genuinely surprised when, the next day, they were asked to write down an account of what had happened.

The event was enacted on three different days in the six schools chosen¹, which were situated in Manchester, Bolton and Liverpool, towns which are sufficiently wide apart to make it unlikely that the news of the experiment would spread. In any one town the event always took place in the different schools on the same day. The same two persons (hereafter referred to² as *A* and *B*) always carried out the event, with none but the very slightest alteration in different

¹ For descriptions of schools and subjects see pp. 394, 395.

² *A* was one of us; *B* was Miss N. Hilton, a graduate research student in the psychological laboratory. We offer her our heartiest thanks for her valuable assistance.

schools (*e.g.* the unavoidable alteration of a colour in the dress or of the position of an object), and with gratifying success. In any case in which an alteration in the rehearsed order was made, it was communicated to one of us, and allowed for in scoring the results.

A time of day was chosen at which the children would be engaged in a singing lesson: this ensured that they had nothing to occupy them when the two persons entered the room. This precaution was necessary in order to avoid the possibility that the children might simply continue their work when the interruption occurred, and take no notice of the event. We can confidently assert that this did not happen. The order of proceedings allowed *B* to observe the children during the whole time that she was in the room, and she reports that all the children took great interest in the doings of the visitors.

The classes were always arranged in a compact group when the interruption occurred, so that the whole event was seen by all, and no child was very far from the performers. The fact that some children were nearer than others could not, of course, be avoided, but the objects figuring in the event were all large, and no questions were asked which referred to details of small objects.

The teacher in charge of the class had been previously told that the event would take place, but that she must not prepare the class in any way, and must act quite naturally. The teachers did not know the exact time at which *A* and *B* would enter; all they knew was that during a certain lesson there would be an interruption, of the general nature of which they were made cognisant. The fact that they themselves knew only in a general way what was to take place contributed, no doubt, to the interest which they took in the proceedings. It also avoided the risk of the hint that an unconsciously *blasé* expression on the teacher's face might have given to the children, had the teachers previously seen the rehearsal of the event.

Description of the dress of A and B.

Dress of *A*.

Navy blue suit.

Fawn coat.

Blue and white striped muffler.

Red and blue striped tie.

Black 'bowler' hat.

Black boots.

Dress of B.

Navy blue costume, black buttons.

Pink blouse with lace over it.

Brown fur and muff.

Brown fur hat, turned up with white, and trimmed with orange velvet and grey feather.

Fawn gloves.

Green bag.

Violets pinned on muff.

Red roses.

Black shoes.

Blue stockings.

Coat well open at the front.

Description of the pre-arranged event. The lesson was interrupted by the entrance of *A* and *B*, who knocked at the door of the class-room, entered, advanced to the teacher in charge of the class, and shook hands. *A* introduced *B* to the teacher in a low tone. The following performance was carried out:

On entering the room, *A* carried a brown bag in his right hand, his black 'bowler' hat and brown gloves in his left hand, and a yellow cane walking-stick over his left arm. After putting his bag on the floor, he shook hands with the teacher and made an introductory remark, "I have brought those things to show you that I told you about." He put his hat, gloves and stick on the piano¹, then placed his bag on the teacher's desk, opened it, and very quickly took out the following articles in the order named below:

A newspaper (*The Daily Citizen*),—thrown out, apparently carelessly, but so that it rested against the bag with its name exposed to the class,—a bunch of keys, a briar pipe, a box of matches, a pocket-knife and a book.

These were taken out of the bag very quickly, and put carelessly on the desk in order to give the impression that *A* was looking for some things which they were covering. The following articles were then taken out more slowly and carefully, and were held up and shown to the teacher, quite naturally, but in such a position that the whole class could see them.

A small flag, consisting of a green oblong background on which was

¹ In the case of Group V this was not done, the articles being deposited elsewhere.

a yellow cross, a coloured statuette of a shepherd boy holding a lamb, a bunch of artificial roses (red and white), and a large paper picture of a cat and a canary, which had the title, "The Cat and the Canary," printed on it in large letters.

The picture was unfolded and shewn to the teacher (and thus to the class, as it was held in such a position that the whole class could see it) for ten seconds.

(The picture was selected from a stock of posters used to advertise kinematograph films at the picture theatres, and was one which had not yet been put into circulation. A picture of this kind was chosen because the items on it were simple, familiar, boldly drawn and distinctly coloured. The newspaper was selected in order to see if the more common names of papers which also have the word 'Daily' as part of their title would be reported instead of the real title.)

As the articles were taken from the bag, appropriate remarks were made in a low tone, such as "How do you like this?" "Will this do?" The articles, after being shown to the teacher, were placed on the desk at the side of the bag, and were afterwards replaced in the bag in the reverse order to that in which they had been taken out. In the meantime *B*, after having been introduced to the teacher, stood at one side of *A*, and assumed a bored expression. (It may be said that her acting was excellent.) She took some violets from her muff, rearranged them, and then replaced them. The two visitors then shook hands with the teacher, said "Good-afternoon" to the teacher and the class, and left the room. The lesson was immediately continued.

The performers were naturally careful to leave the school premises as quickly as possible, and to arrive only a minute before the event took place, in order to avoid being seen by children other than those concerned in the experiment. One of them started a concealed stop-watch as they entered the room, and stopped it as the event finished, so that the duration of the event was known. It varied between 2 minutes 10 seconds and 2 minutes 35 seconds.

The room was always chosen so that those entering it could not be seen approaching the glass door, and would not be visible until they had completely entered the room. In every case the event took place during the afternoon session of the school.

The presence of *B* served several purposes. Besides tending to provide a counter-attraction for the onlookers, she added many details of dress and appearance which could be used as the basis of testimony, and, being relatively unoccupied, she was able to watch the children

unobtrusively, and to report anything of interest in the behaviour of the class.

The Subjects' attitude towards the experiments. From the evidence of the teachers, quoted on p. 390, we know that the children did not suspect a test. From our general impression gained from conversation with the teachers, it does not appear that the event created any extraordinary amount of interest amongst the children, or that they spoke about it much amongst themselves when they dispersed or reassembled the next morning. The fact that the event was followed by a period during which they were in the presence of the teacher prevented them from comparing notes, to any great extent, directly after the event.

As the testimony was taken early in the morning after the event, we may then conclude (and in this we are supported by the statements of the children themselves) that very little leakage of the account from one child to another had taken place. The usual impression of the occurrence seems to have been that "the gentleman wanted to sell the things," or (in the defective schools) that he "had made them." After the children knew that they had been victims of a plot, there was naturally an exchange of recollections, and this we have investigated by suddenly requiring the testimony of the children again, seven weeks later. In the case of any event of which children might be required to give evidence it is often difficult to avoid the effect of knowledge of the experience of the same event by others, and the value of an estimate of the reliability of their own impressions, before and after consultation with others, is obvious.

III. SUBJECTS OF THE EXPERIMENTS.

The results of this investigation were obtained from two schools for normal children and four schools for mentally defective children¹, the six different groups being constituted as follows:

Group I. 39 normal children, of both sexes, attending the Fielden Demonstration School, Manchester. Their ages ranged from 11 to 14 years, and in social status they were slightly superior to the average scholar of the elementary school. At this school modifications in the curriculum constantly occur, and visitors frequently enter the class-

¹ *I.e.* children of such a degree of mental subnormality that they had been adjudged to require teaching in 'special' schools (in the sense usually attached to this word in England).

rooms, consequently the occurrence of an event such as that which formed the material of this investigation would be expected to cause no undue excitement. As a 'control' experiment, an elementary school in which the curriculum and arrangements are more constant was visited, viz. the school mentioned under II.

Group II. 26 normal girls, ages 11 to 12 years, at the Clarendon Street Council School, Bolton.

Group III. 28 mentally defective children, 14 girls and 14 boys, ages 11 to 14 years, at the Chatham Place Special School, Liverpool.

Group IV. 20 mentally defective children, 15 boys and 5 girls, ages 11 to 13 years, at the Orwell Road Special School, Liverpool.

Group V. 16 mentally defective boys, ages 11 to 14 years, at the Flash Street Special School, Bolton.

Group VI. 14 mentally defective boys, ages 10 to 13 years, at the Kay Street Special School, Bolton¹.

Since most of the children of the different groups fall within the same age limits, it will be possible to compare one group with another with respect to the effect of the different conditions of life upon their powers of giving testimony.

IV. THE CHILDREN'S TESTIMONY.

The 'narrative' and the 'interrogatory.' It may be mentioned that the above two terms are now frequently used in connexion with this work. The narrative (corresponding to the *Bericht* of Stern) is the account given by the subject when he is allowed to proceed in his own way, unhampered by questions or by any personal influence. In the interrogatory (the *Verhör* of Stern), on the other hand, he is required to give answers to a set of pre-arranged questions, which are read to him by the experimenter, and some of which may be suggestive in varying degrees.

In the present experiments a period of 19½ hours (a period found to be most convenient in the case of the first school, and so kept constant in the subsequent experiments) was allowed to elapse after the event, then the teacher, without preliminary warning, said to the children:

"I want you to write an account of *everything* you saw from the

¹ We offer our very hearty thanks to the head masters and head mistresses of these schools for their kind permission to carry out the experiments, and to the class teachers for their valuable co-operation in obtaining the results.

time the lady and gentleman entered the room to the time they went out."

When all had completed the narrative, the papers were collected and the children instructed as follows:

"I am now going to ask you a number of questions. You will probably not be able to answer all the questions, but some of you will be able to answer more than others. When you are unable to give an answer, leave a space where the answer should be. The answers must be as short as possible."

The words used by every teacher, and the order of proceedings had been fixed by us, and instructions for carrying on the tests were in the teacher's hands on the morning after the event, so that the conditions of the test were the same in each school.

V. TREATMENT OF THE RESULTS.

In the evaluation of the results, an 'item' was taken as the unit of measurement, and an item was defined as any particular piece of information about the event. Thus, in the phrase, 'A brown bag on the table,' there are four items of information. It was thus possible to give exact numerical expression to the *range* (the number of items mentioned whether correct or incorrect) and the *accuracy* (the number of items correct) of the narrative and the interrogatory.

In both the narrative and the interrogatory, one mark was given for each item correctly expressed, and a similar value was awarded to each incorrect item.

An event may conveniently be divided into categories, and accordingly the following were selected as relevant to the aims of the investigation: items, colours, shapes, sizes, position, action, sequence, and number.

VI. THE RESULTS OBTAINED.

A. *The Narrative.*

The mean and standard deviations¹ (σ) for each of the groups are given in the following table. The value for the range and number correct are given separately. (For the description of Groups I, II, etc. in this and subsequent tables see pp. 394, 395.)

¹ The standard deviation is the square-root of the mean of the squares of the deviations of the separate values from the mean of all the values, and is a measure of the degree of scatter of the separate values about their mean.

Group	Number of cases	Range		Number correct		Percentage correct
		Mean	σ	Mean	σ	
I	39	47.7	30.2	46.0	27.2	96.3
II	26	74.8	19.6	72.0	18.9	96.1
III	28	24.7	8.6	23.1	8.5	93.1
IV	20	16.9	6.0	16.3	5.9	96.3
V	16	31.0	16.5	30.1	15.5	97.0
VI	14	18.1	7.9	17.7	7.8	97.7

It will be seen that in every group the degree of accuracy attained is remarkably high, and hence the *spontaneous* account of an event is exceedingly reliable, even in the case of mental defectives. In many respects, not a single deviation from the actual situation was to be found. Thus, when the testimony of children is unaffected by questions or suggestions, it is worthy of the utmost consideration.

However, the amount proffered by the different subjects varies considerably, and in no case is the range very extensive. A glance at the table will show that the girls of Group II give the most detailed account of the proceedings (mean, 74.8); the reports of the children of Group I are much 'thinner' (mean, 47.7). The mentally defectives tender still shorter accounts of the event; the means of Groups III, IV, V, and VI being respectively 24.7, 16.9, 31.0, and 18.1. In this respect the spontaneous account given by the mentally defectives is much less valuable than that given by the normal children. Such quantitative difference provides a means whereby the two classes of children may be distinguished from one another. Individual differences in the range of the report are generally well marked; this is especially noticeable in the children of Group I ($\sigma = 30.2$). Although the possibility of individual variations is greatest in Group II, since this group yields the highest mean, the standard deviation is relatively small (19.6); a value which shows that the performances of the girls of Group II are steadier and fluctuate less than the children of Group I. This may be due in part to the different methods of teaching employed at the two schools. The more rigid discipline to which the girls of Group II are subjected may tend to produce a greater degree of uniformity in the results than the freer discipline which prevails at the Fielden Demonstration School.

Classification into Categories. The following table shows the separation of the results into the categories mentioned on p. 396.

The upper line in each group represents the mean number correct

per subject in each category; the lower line gives this value as a percentage of the total number correct in all the categories.

Group	Items	Colours	Shapes	Sizes	Position	Action	Personal items	Sequence	Material	Number
I	12.1	2.7	.9	1.7	4.5	15.4	3.2	3.9	.8	.7
	26.4	5.9	2.0	3.7	9.8	33.6	6.9	8.5	1.8	1.5
II	10.6	5.3	1.4	1.8	7.3	25.0	13.9	3.9	2.3	.5
	14.8	7.5	2.0	2.4	10.1	34.7	19.2	5.4	3.1	.7
Mean*	11.3	4.0	1.2	1.8	5.9	20.2	8.5	3.9	1.6	.6
	20.6	6.7	2.0	3.1	9.9	34.1	13.1	6.9	2.5	1.1
III	7.0	.18	.11	.14	4.1	8.6	2.3	.61	.18	—
	30.2	.7	.5	.6	17.6	37.0	9.9	2.6	.7	—
IV	5.4	.2	.05	.15	1.6	6.4	2.0	.3	.1	.05
	33.1	1.2	.3	.9	9.8	39.3	12.3	1.8	.6	.3
V	8.8	.62	.12	.18	2.8	11.6	5.0	.75	.24	—
	29.1	2.1	.4	.6	9.4	38.5	16.6	2.5	.8	—
VI	5.6	.07	—	.07	2.6	4.9	3.7	.29	.29	.07
	31.5	.4	—	.4	14.9	28.2	20.9	1.6	1.6	.4
Mean*	6.7	.27	.09	.13	2.8	7.9	3.2	.49	.20	.06
	31.0	1.1	.4	.6	12.9	36.0	14.9	2.1	.9	.3

* In evaluating the means of the different groups no allowance has been made for the different numbers of subjects in the groups.

From a consideration of the above table it is at once evident that certain components of the event appeal to the children more than others. Over one-third of the items enumerated are included in the category of action, a fact which tends to show that children are primarily interested in the moving aspects of the situation. This illustrates very clearly the defects of a *picture* as material for a testimony experiment, since it may fail to stimulate the subject's interest in movement. Another category which attracted a prominent share of the subject's interest is that of 'items.' For convenience, and also in order to effect a more complete analysis, this category was divided into items connected with the persons (these included articles of dress and personal features), and into items not directly associated with *A* and *B*, such as articles taken from the bag. The only other category which is noticeably prominent is that of 'position.' The remainder of the narrative is very insignificant; there is a remarkable lack of evidence upon such categories as colour, shape, and number, particularly in the case of the mentally defectives. 95 % of the evidence of these mentally defective subjects is made up of

a description of actions, items, and positions; the remaining 5% is divided between all the other categories. These three categories constitute over three-fourths of the narrative of the normal children, though with this class of children the remaining categories assume a relatively more important position. The fact that in the case of the normal children, only 6.7% of the description deals with colours is remarkable when one considers that colours formed such an apparently prominent part of the event. Possibly the name of a familiar object tends to be given in preference to its colour, shape, or size, and only when the object is unfamiliar do these categories receive relatively more attention.

There are several points of difference between the narratives of the normal children and those of the mentally defectives. As we pass from the narratives of the defective to those of the normal children, such components as colours, sizes, shapes, etc. are found to be mentioned more frequently. Also, those defectives who produce the most intelligent narratives begin to note these categories. Further, the mentally defectives usually describe those parts of the event in which they are most interested, independently of their chronological order. The normal children, on the other hand, almost invariably describe the event in the order in which it took place.

The narrative of the defectives is often fragmentary and disconnected and repetitions frequently occur. Often the transition from one sentence to another is very abrupt.

Some of the descriptions given by the mentally defectives throw much light upon the condition of the minds of these subjects. In one case, it was said that the bunch of keys taken from the bag was used to wind up the gentleman's watch. The canary in the cage was often described as a "poll-parrot." To one, the flag was "a piece of cloth," and to another, "a cross on a piece of cloth." One subject even went so far as to state that she "saw a book with lots of pictures in it," although the book in question was never opened. The normal children do not show such wide deviations as these, yet occasionally imaginative statements are made.

A feature of the papers of the normal children, and especially of those of the girls of Group II, is the aesthetic appreciation of the appearance of *A* and *B*. It was often said that *B*'s clothes suited her; that her clothes were very nice and she looked nice to speak to. One girl in Group II was evidently very much impressed, for she states that "they were exquisitely dressed, and the lady's clothes and colours harmonized with each other. They were not very old, but were very

graceful and polite, and when they stood beside each other they went very well."

Distribution of Interest between A and B. The results of the narrative were further arranged to show the distribution of interest between *A* and *B*. The following table shows the extent of this distribution.

		Group I	Group II	Group III	Group IV	Group V	Group VI
Number correct	<i>A</i>	14.4	22.7	7.4	4.9	7.4	5.4
	<i>B</i>	7.2	25.7	5.4	4.0	5.3	4.0

It will be seen, that with the exception of Group II, more information is given about *A* than about *B*. The description of *A* given by the children of Group I is twice the length of the description of *B*. In order to ascertain if the difference between Groups I and II were due to the presence of boys in Group I, the values for the boys and girls of this group were evaluated separately and were found to be as follows:

	<i>A</i>	<i>B</i>
Number correct per girl ...	13.0	7.0
Number correct per boy ...	15.8	7.4

Thus the description of *A* given by the girls is shorter than that given by the boys, but it cannot be said that an appreciable sex difference in interest exists here.

The girls of Group II give equally good descriptions of *A* and *B*. If the results of the children of Group III, which contains 14 girls and 14 boys (mental defectives), are treated separately, we find that the descriptions of *A* and *B* given by the boys are longer than those given by the girls. The averages for the boys are 8.4 and 6.6 respectively; the corresponding values for the girls are 6.3 and 4.1.

B. *The Interrogatory.*

Range and Accuracy. The questions asked were of a most comprehensive nature, and dealt with all the aspects of the event. They were given orally, in order to minimise the temptation which would have arisen if the questions had been printed, to look back and answer any question which had been passed over, when the answer to it had been suggested by a question occurring later in the series.

In the following table will be found the average number of correct and incorrect replies, together with the mean variation (M.V.) for each of the groups tested¹.

Group	No. of cases	Number correct				Number incorrect			
		Mean		M.V.		Mean		M.V.	
		1st test	2nd test	1st test	2nd test	1st test	2nd test	1st test	2nd test
I	39	73.2	68.2	7.8	7.0	49.1	55.0	9.8	11.4
II	26	64.0	63.4	9.8	8.3	31.5	42.1	12.7	12.4
III	24	48.2	49.5	11.6	7.8	54.1	63.6	11.1	15.7
IV	19	54.8	47.4	5.8	5.5	81.7	80.8	8.3	5.3

It is at once evident from this table that the children of the different groups show considerable variation in the number of questions answered. Some children give an answer to every question; this may be the outcome of the desire to excel or to please, but it has, of course, a disastrous effect upon the reliability of their evidence. Others are more critical and cautious, and consequently fail to answer many of the questions.

A comparison of the number of correct and incorrect replies gives an indication of the accuracy and hence of the reliability of the interrogatory in general. It will be remembered that in the narrative the number of errors was almost negligible. This condition, however, does not obtain in the interrogatory. Over one-third of the replies of the normal children and over one-half of the replies of the mentally defectives are incorrect. Thus the interrogatory of the latter group is very unreliable, and the corresponding testimony of the normal children must be treated with great reserve.

It is interesting to note the inversion which takes place in passing from the normal children to the mentally defectives. In the case of the former group the majority of the replies are correct; in the latter, the correct replies are in the minority.

Repetition of the interrogatory after an interval of seven weeks. The interval of seven weeks which elapsed between the two interrogatories had only a slight effect upon the children's memory of the event. As might be expected, the accuracy of the second interrogatory was less

¹ Groups V and VI are not included in this table as their results in the interrogatory were rendered useless by a slight misunderstanding of the directions.

than that of the first, the decrease in the number of correct replies being accompanied by an increase in the number of incorrect replies, but not to the extent that might have been anticipated. Still, the increase in the number of incorrect replies is correspondingly greater than the decrease in the number of correct replies. Thus, what was uncertain in the first place becomes still more uncertain later, but what was distinctly perceived suffers little at the hands of time¹.

Classification into categories. The table on page 403 shows the division of the replies into categories.

The upper row of each category represents the mean value per subject. The lower row gives this value as a percentage of the total number possible in the columns headed 'Number Answered,' and as a percentage of the number answered in the columns headed 'Number Correct.'

Range. The percentage number of questions answered in the different categories is fairly uniform. In the case of the normal children it ranges from 60 to 80 %. The questions which refer to actions are answered most frequently; the next in order of frequency are those which relate to personal items and positions. The defective children respond most frequently to the suggestive questions (85.8 %), and to a slightly less extent to those concerning colours (81.8 %), actions (78.6 %), and position (77.7 %). The tendency to answer every question is well illustrated in the replies of the defectives of Group IV. The mean number of questions answered reaches the high value of 81.7 %, and in the categories of suggestion and colour (which contain the greatest number of questions) the values are 96.1 % and 91.6 % respectively. Even the normal children of Group I reply to 77.0 % of the total questions. The most cautious children are the girls of Group II, who reply to only 60.6 % of the questions.

When the interrogatory was repeated after an interval of seven weeks, there was a slight increase in the number of questions answered by the normal children.

Accuracy. It will be remembered that over one-third of the narrative dealt with the *active aspect* of the event; this category is also prominent in answers given to the interrogatory. 84.0 % of the replies

¹ K. M. Dallenbach, in an article on "The Relation of Memory Error to Time Interval" (*Psychol. Rev.* 1913, xx, 323-337), concludes, as the results of experiments with pictures, that the memory error increases with the time interval, very rapidly at first and then more gradually as the time interval becomes greater. The percentage error after 45 days was found to be 22.4 for one picture and 18.1 for another. His methods differ considerably from ours.

INTERROGATORY.

Category	Number possible	Number answered										Number correct													
		1st test					2nd test (after 7 weeks)					1st test					2nd test (after 7 weeks)								
		I	II	Mean	III	IV	I	II	Mean	III	IV	Mean	I	II	Mean	III	IV	Mean	I	II	Mean	III	IV	Mean	
Items	25	17.1	13.2	15.2	12.7	21.2	16.9	17.6	15.0	16.3	15.3	16.3	15.8	12.8	8.9	10.8	7.2	10.7	9.0	11.7	10.7	11.2	8.6	7.8	8.2
Colours	39	68.4	52.8	60.6	50.8	84.8	67.8	70.4	60.0	61.2	65.2	63.2	74.8	74.8	67.4	71.1	56.3	50.5	53.4	66.5	71.5	69.0	56.2	47.8	52.0
Shapes	6	28.7	19.8	24.3	28.1	35.7	31.9	28.8	23.9	26.3	29.7	35.5	32.6	15.6	11.8	13.7	11.4	13.0	12.2	14.8	13.4	13.6	11.1	10.1	10.6
Sizes	14	73.7	51.1	62.4	72.1	91.6	81.8	73.9	61.3	67.6	76.2	91.1	83.6	54.4	59.1	56.7	40.5	36.4	38.4	51.2	51.9	52.5	37.3	28.5	32.9
Position	11	4.8	3.9	4.3	3.3	3.7	3.5	4.5	4.0	4.2	4.0	3.7	3.9	83.7	87.2	85.4	72.4	86.5	79.4	74.3	85.0	79.6	67.5	70.3	68.9
Action	14	79.6	65.4	72.5	55.0	61.7	58.3	74.5	66.6	70.5	66.6	61.6	64.1	83.7	87.2	85.4	72.4	86.5	79.4	74.3	85.0	79.6	67.5	70.3	68.9
Personal items	20	12.2	8.8	10.5	—	—	—	12.3	10.1	11.2	—	—	—	6.4	4.1	5.2	—	—	—	6.4	5.7	6.0	—	—	—
Sequence	10	78.1	54.8	66.4	7.7	9.4	8.5	77.0	72.2	74.6	7.8	8.3	8.0	52.1	46.9	49.6	4.0	3.8	3.9	51.8	56.4	54.1	3.7	3.2	3.4
Number	3	79.8	67.1	73.4	70.0	85.5	77.7	83.4	76.4	79.9	70.9	75.4	73.2	58.7	58.3	58.5	52.7	40.4	46.5	59.6	54.8	57.2	47.4	38.6	43.0
Suggestion	36	11.7	11.2	11.5	9.8	12.2	11.0	11.9	11.6	11.8	10.6	11.8	11.2	75.7	79.7	84.2	6.4	6.5	6.5	78.9	80.4	9.6	7.4	7.5	7.5
Mean		83.9	80.1	82.0	70.0	87.2	78.6	85.3	82.8	84.0	75.7	84.3	80.0	75.7	79.7	84.2	69.7	53.3	61.5	83.8	80.4	81.8	69.8	63.6	66.7
		16.5	13.8	15.1	11.0	14.2	12.6	15.6	14.9	15.3	12.4	13.7	13.0	11.8	10.8	11.3	7.6	9.3	8.4	12.7	12.5	12.6	8.9	9.2	9.1
		82.5	69.0	75.8	55.0	71.0	63.6	78.0	74.5	76.2	62.0	68.5	65.2	71.5	78.2	74.9	69.1	65.5	67.3	80.1	83.3	81.9	71.8	67.2	69.5
		7.3	4.5	5.9	4.8	7.6	6.2	6.6	6.1	6.3	5.3	6.8	6.0	2.6	7	1.7	9	1.4	1.2	1.8	2.0	1.9	1.0	5	7
		73.0	45.0	59.0	48.0	76.0	62.0	66.0	61.0	63.0	53.0	68.0	60.5	36.3	14.5	25.4	18.5	18.4	18.4	27.2	32.8	30.0	18.8	7.4	13.1
		2.1	1.9	2.0	—	—	—	2.3	1.0	1.7	—	—	—	50.0	50.0	50.0	—	—	—	46.8	50.0	48.4	—	—	—
		71.7	61.5	66.6	—	—	—	77.4	33.3	55.3	—	—	—	15.4	16.0	15.7	12.4	12.3	12.4	11.6	11.9	11.8	10.7	10.2	10.4
		28.5	21.4	25.0	27.3	34.6	30.9	27.6	23.7	25.7	29.2	32.3	30.7	53.8	74.9	64.3	45.6	35.5	40.5	41.4	50.0	45.7	36.6	31.6	34.1
		79.2	59.5	69.3	75.6	96.1	85.8	77.5	65.8	71.6	81.1	89.7	85.4	53.8	74.9	64.3	45.6	35.5	40.5	41.4	50.0	45.7	36.6	31.6	34.1
Mean		77.0	60.6	68.8	62.1	81.7	71.9	76.3	65.4	70.8	68.3	75.5	71.9	61.1	62.9	62.0	53.1	48.3	50.7	57.2	62.6	60.0	50.7	44.4	47.5

of the normal children in this category are correct; in the case of the girls of Group II the accuracy reaches the exceedingly high value of 93·8 %. In the mentally defectives only 61·5 % were correct for this category. Thus the importance of *movement* in material for investigating testimony receives additional emphasis.

This resemblance of the interrogatory to the narrative is further increased in the category of *items*. Three-quarters of the replies to the questions relating to 'personal items' are correct; the answers to the questions dealing with the remaining items are slightly more inaccurate. These results again show that the attention of the children tends to be focussed upon the personal factors of the event.

Throughout this investigation, a noticeable feature was the unreliability of the evidence in connexion with *colours*¹. Only 56·7 % of the replies of the normal children are correct; and the evidence of the defectives is still more inaccurate (38·4 %). Unless the colours are strikingly prominent, they are seldom noticed.

The evidence relating to the *sequence of events* was very unreliable; only one-quarter of the replies given being correct. It should be stated, however, that the questions in this category dealt chiefly with the order in which the articles were taken out of the bag, and as this process was somewhat rapid, it evidently passed unnoticed by the children. Still, the duration of this process occupied the greater part of the event; and yet the evidence upon it is almost worthless.

In all the categories, the replies of the defective children are less accurate than those of the normal children; the respective mean values for these two classes being 62·0 % and 51·0 %. In every category, some components pass quite unperceived, whilst others are observed by everyone.

The effect of *repeating the questions after the interval of seven weeks* is to produce only a slight decrease in the accuracy of the replies. This decrease amounts to 3 % and 6 % for the normal and defective children respectively. Thus the increase in the number of questions answered is accompanied by a decrease in the accuracy of the replies.

The Categories in detail.

A. *Items*. The items directly connected with *A* and *B* are enumerated with greater accuracy than those of more remote connexion with them. This tends to show that the children were primarily interested in the persons and their possessions. It is curious to note the

¹ Also noted by Dallenbach, *op. cit.*, Whipple, *op. cit.* p. 308.

gradual transition from certainty to uncertainty which occurs as the object in question becomes less and less prominent. The following questions and replies illustrate this point:

Questions	Correct	Incorrect
Had the lady a fur?	59	8
Had the lady a muff?	58	9
Did the lady wear eyeglasses?	50	14
Had the lady a bag?	41	17
Was the lady wearing a rose?	40	22

Further, what is irrelevant to the whole situation, providing it is not too incongruous, generally passes unnoticed. Thus the children were shown a picture of a cat and a canary, upon which was an objectively very prominent trade mark consisting of the letter *S* on a diamond-shaped blue background. This was perceived by six children only; evidently the interest of the children was centred upon those aspects of the picture which gave it its meaning.

None of the children succeeded in enumerating correctly the names of all the articles which *A* took from the bag. With the exception of the flag, flowers, statuette, and picture, the articles were just taken out of the bag and immediately placed on the desk, and hence attracted the attention of the children for a comparatively short time. This may account for the fact that the articles were occasionally said to include such objects as a cigar-holder, pouch, tobacco, string, scissors, letters, kite, balloon, hair-tidies, handkerchief, looking-glass, mouth-organ, and railway-guide.

It is interesting to note that though the children were generally aware that there were initials on *A*'s bag, they failed to notice what the letters actually were.

The various ways in which the statuette was perceived produced some interesting results. It was a representation of a curly-haired shepherd boy holding a lamb, but it was described as such by only eight subjects.

Others perceived it as a girl, a lady, a man, a boy, a doll, a soldier, a musician, or as Britannia, Justice, Jesus and His Mother, Holy Mary, an angel, Nelson, or Florence Nightingale. The figure was said to be holding a spear, a picture, a cross, a crook, a head, a vessel, a baby, a jug, flowers, knitting, or a lamp.

Thus, when the objects are inattentively perceived, the individual differences in interpretation are considerable.

B. *Colours.* Usually a colour had to be intensely prominent if it were to be accurately perceived. Thus the colour of *A*'s muffler was described by the normal children as white (16), green (10), blue (14), grey (3), yellow (2), black (2), brown (2), and in Manchester as the Manchester University colours: blue, green, and silver (3). The actual colours present were blue and white. His tie was depicted as an object of still more varied hues. On the other hand an article such as *B*'s dress generally evoked unanimous replies; 49 normal children gave it correctly as blue and eight others stated that it was black. Whenever there is any uncertainty as to the existence of an object, this uncertainty is accentuated in the responses to the questions about its colour.

It appears evident that the children often receive impressions of 'lightness' or 'darkness,' and not of the actual colour. For instance, *B*'s muff and fur were in reality brown in colour; in the majority of cases the colour was given as black, much less frequently as brown. Only very occasionally was it given as grey or white. Often, though an object was correctly perceived, its colour passed unnoticed. The title of the picture, "The Cat and the Canary," was printed in large red type. Forty-six of the normal children gave the correct title, but only nine were able to name the colour of the letters. Other colours given were: black (19), yellow (5), white (5), gold (3), blue (3), brown (1), and green (1).

People are by no means agreed on the question of the reliability of evidence upon the colour of hair. In the present investigation there is much more agreement than difference. *A*'s hair was decidedly fair; the actual colours given were light (28), brown (16), golden (5), white (3), red (2), grey (1), yellow (1), sandy (2), silvery (1), and 'ginger' (1). It should be noted that in this case the colour black was never mentioned, which again supports the contention that the normal children generally receive a distinct impression of 'lightness' or 'darkness' as the case may be. The defective children, however, show some variation from the above situation. Sixteen of these children state that *A*'s hair was black, in fact a dark colour was given more frequently than a light colour.

Thus many erroneous statements are made in connexion with the colours of objects, and a careful consideration of the factors at work is necessary before such evidence is accepted as correct. As a rule, in the observation of such events, colours do not seem to be in the focus of attention.

C. *Sizes.* The defective children were entirely unable to estimate the sizes of the components of the event. Their answers showed that they had no conception of the dimensions of such objects as A's bag or the picture, and in this respect they exhibited a remarkable difference from the normal children. Consequently the following remarks relate only to the normal group.

The following table gives the results in detail:

	Group I			Group II			Mean of I and II	Correct value
	Number of cases	Mean	M.V.	Number of cases	Mean	M.V.		
		ins.	ins.		ins.	ins.	ins.	ins.
Length of bag	39	25.2	5.7	21	19.2	5.5	22.0	18.0
Breadth of bag	36	12.3	2.4	22	10.4	3.8	11.3	12.0
Height of bag	35	13.1	3.6	17	10.5	3.7	11.8	8.0
Height of man	37	65	5.0	13	62	7.0	63.5	68.5
Length of picture	39	36.2	7.2	16	31.1	9.0	33.6	36.0
Breadth of picture	39	24.3	6.1	13	22.2	5.1	23.2	24.0
Height of statuette	38	9.1	1.8	21	8.5	2.4	8.8	9.0
Length of book	26	8.4	1.9	11	8.9	2.0	8.7	9.0
Width of book	25	5.3	.8	10	5.7	1.9	5.5	6.0
Length of flag	33	7.6	2.1	17	7.2	2.2	7.4	9.0
Width of flag	33	4.8	1.3	16	4.7	1.2	4.7	6.0

D. *Duration of the Event.* The children were asked to estimate the length of time over which the event lasted, and the question gave rise to some very interesting results. The actual time occupied by the event varied from 2 mins. 10 secs. to 2 mins. 35 secs. at the different schools; the following table gives the results obtained from Groups I and II.

	Group I		Group II	
	Mean	M.V.	Mean	M.V.
1st test	8.1 mins.	3.2	11.0 mins.	3.0
2nd test	9.5 „	2.5	10.6 mins.	2.5

As in the previous case, the defective children evinced no idea of the length of the time-interval; and, in the few instances in which estimations were expressed, they showed no resemblance whatever to the correct value. Estimations ranging from half-an-hour to an hour were very common.

In every case, the time-interval was enormously over-estimated¹; an example of the familiar case of the 'filled interval'². After a period of seven weeks the interval is still further over-estimated by the children of Group I, but the estimations of the girls of Group II are slightly less erroneous. This interval between the tests also tends to produce more agreement between the results, since there is a considerable reduction in the mean variation.

The over-estimation by the girls of Group II is greater than that by the children of Group I. It will be remembered that the narrative of the former group was longer and included more details than that of the latter, hence the difference in the over-estimation of the time-interval may be due to the fact that the interval appeared to be more 'filled' to Group II than to Group I.

A separate treatment of the time-estimations of the boys and girls of Group I reveals a slight superiority in accuracy of the boys over the girls. The actual values are:

				Mean	M.V.
Boys (21)	7.7 mins.	3.1 mins.
Girls (20)	8.5 "	3.3 "

E. *Sequence of the Event.* The evidence in this category is most unreliable; the replies of the defective children being much less satisfactory than those of the normal children. The majority of the children state that the articles were taken out in the following order³:

	1	2	3	4	5	6	7	8	9	10
Group I.	Pr.	Pe.	Ks.	Fg.	Fs.	Pt.	Pt.	Fs.	Pt.	Pt.
„ II.	S.	S.	Fs.	Pt.	Pt.	Pt.	Fg.	Fg.	Pt.	Pt.
„ III.	S.	S.	Pt.	Fs.	Ks.	Pt.	Ks.	Pt.	Fs.	Fs.
„ IV.	Fg.	S.	Pe.	Pt.	Pr.	Ke.	B.	Pe.	Pt.	Pt.

(Incorrect replies are italicised.)

Pr.=Picture; Pe.=Pipe; Ks.=Keys; Fg.=Flag; Fs.=Flowers; S.=Statuette;
B.=Book; Pr.=Paper; Ke.=Knife.

Thus the table consists almost entirely of the Statuette, Flowers, Flag, and Picture. These were the objects which received the longest

¹ Similar cases of over-estimation have been recorded by J. Dauber: "Die Gleichförmigkeit des psychischen Geschehens und die Zeugenaussagen," *Fortschritte d. Psychol.* 1912, i. 2, S. 102; by W. Stern, *Beitr. z. Psychol. d. Aussage*, 1904, ii. 1, S. 32 ff. and 57; by R. Oppenheim, *ibid.* 1905, ii. 3, 75 ff.; by O. Lipmann, *Ztsch. f. angew. Psychol.* 1911, iv. S. 312 f. and also by H. Breukink, "Ueber die Erziehbarkeit der Aussage," *Ztsch. f. angew. Psychol.* 1909, 32-87.

² When an interval is occupied by auditory, visual, or tactile stimuli, this filled interval appears longer than an 'empty' interval of the same length, unless the interval is very long.

³ The actual order is given on p. 392.

exposures, and the children's attention was dominated by them. Hence as regards the sequence of events, only those objects which receive additional emphasis are noted with an appreciable degree of accuracy.

Comparison of the replies relating to A and B respectively.

In the following table, the upper row of each category represents the mean value per subject. The lower row gives this value as a percentage of the total number possible in the columns headed 'Number answered,' and as a percentage of the number answered in the columns headed 'Number correct.'

Group	First test				Second test			
	Number answered		Number correct		Number answered		Number correct	
	A	B	A	B	A	B	A	B
I	26.7	24.4	18.4	15.5	25.4	25.0	17.4	16.0
II	80.9	81.3	68.8	63.6	76.8	83.4	68.5	63.8
Mean	22.1	21.0	16.1	15.0	23.2	22.9	16.9	16.5
	66.9	70.0	75.3	72.1	70.3	76.3	72.8	72.0
	24.4	22.7	17.3	15.2	24.3	24.0	17.2	16.2
	73.9	75.6	72.0	67.8	73.6	79.8	70.6	67.9
III	17.2	23.1	10.1	11.9	18.0	23.8	10.6	12.7
IV	52.1	77.0	58.7	51.5	54.5	79.3	58.9	53.4
Mean	22.0	26.2	12.5	11.9	21.3	26.6	11.6	12.4
	66.6	87.3	56.9	45.4	64.5	88.7	54.4	46.6
	19.6	24.6	11.3	11.9	19.6	25.2	11.1	12.6
	59.3	82.1	57.8	48.4	59.5	84.0	56.6	50.0

Thus the questions relating to *A* are answered, on the whole, more frequently, but less accurately, than those referring to *B*, and hence we are justified in assuming that the children were more interested in *A* and what he did, than in *B* and her actions. This is true for both the normal and defective children. The relative inaccuracy of the defective children is again prominent in this aspect of the investigation.

The results of the boys and girls of the different groups were also separately tabulated, but there was no evidence of any appreciable sex differences. There was a slight tendency for the boys to be more accurate than the girls of the same group in the replies in connexion with *A*.

Suggestibility of the Subjects.

Of the 150 questions which constituted the interrogatory, 36 were framed to test the suggestibility of the subjects. The questions dealt with all the more important aspects of the event, and were of varying degrees of subtlety. The results obtained are given in the following table :

	First test						Second test					
	I	II	Mean	III	IV	Mean	I	II	Mean	III	IV	Mean
Number answered (<i>A</i>)	28·5	21·4	24·9	27·3	34·6	30·6	27·9	23·7	25·8	29·2	32·3	30·7
Number correct (<i>C</i>)	15·4	16·0	15·7	12·6	12·3	12·5	11·6	11·9	11·8	10·7	10·2	10·5
Accuracy = $\frac{C}{A}$	·54	·75	·64	·45	·35	·40	·42	·50	·46	·37	·31	·34

Thus, as a rule, the children are susceptible to suggestions; the normal children to the extent of 36 %, and the defectives to the extent of 60 %. Hence the latter are much more liable to suggestion than the former, they exercise very little judgment, and their attitude is decidedly uncritical. In the second test there is a noticeable increase in the susceptibility to suggestion, the percentage value increasing from 36 to 54 in the case of the normal children. Consequently the ability to resist suggestion depends in part upon the freshness of memory of the material to which the suggestions relate. There is no evidence to show that this difference in suggestibility between Groups I and II is due to the presence of the two sexes in Group I. The children of Group I also give more varied replies than the girls of Group II. Thus, when asked the question: "On which part of *A*'s face was there a cut?" the girls of Group II, who answered the question, gave: none (7), near eye (1), and on neck (2); but the replies of the children of Group I were as follow: none (4), chin (3), right cheek (8), left cheek (5), near eye (1), forehead (2), neck (1), and over eye (1).

Correlations between resistance to suggestion, intelligence and age.

Coefficients of correlation were calculated between the accuracy of the replies to the suggestive questions and general intelligence¹ (teacher's estimate). The coefficients for Groups I and II are .03 and -.13 respectively, showing the absence of relation between intelligence and suggestibility. However, there is a small positive correlation between age and ability to resist suggestion. For Group I the coefficient is .24; hence, so far as the evidence warrants, the older the child the less suggestible he tends to be.

Effect of previous experience. Knowledge of similar situations previously encountered has a considerable influence upon the replies given. Components of the event which are vaguely perceived, or not perceived at all, are often interpreted or supplied according to the manner in which they are most usually experienced. Thus, in almost every case, the colour of the handkerchief in A's overcoat pocket was said to be white (66). Other colours, rarely given, were pink (1), brown (2), green (1), yellow (1), red (3), and blue (4). As no handkerchief was to be seen it is evident that the children acted upon the knowledge that the most usual colour of such an article is white.

Further, the relevancy of a situation determines in part the nature of the replies given. A's walking stick was an ordinary cheap, light yellow cane. A gold band on such a stick would be decidedly out of place, hence we find the children stating that a silver band adorned the stick, although the stick did not possess a band of any description. Similarly, the general appearance of A suggested to the children that he was worthy of a gold watch-chain, and not one of silver or of brass, yet in reality his watch-chain could not be seen.

The effect of the range of the child's experience upon his evidence is well illustrated in the answers to the question: "What was the name of the newspaper?" The replies of the Manchester children include *The Guardian*, *City News*, *Courier*, *Evening Chronicle*, and *Evening News*. The Bolton children gave similar names, since most of the papers are common to the two towns. In the replies of the Liverpool children are to be found the names of such local papers as *The Echo*, *Express*, and *Mercury*.

¹ By means of the product-moment formula.

Comparison of the Suggestiveness of the Questions.

By ascertaining the number of correct and incorrect replies given to the suggestive questions, it is possible to arrange these questions in the order of their suggestiveness. Such an arrangement shows that the most suggestive questions refer to those components of the event which are not prominent or which are indefinitely perceived, and particularly to those suggested components which might be expected to exist. The cases of the silver band on the walking-stick and the gold watch-chain are examples.

When asked if the figure of the statuette was wearing sandals or boots, 21 normal children gave sandals, 11 boots, and only one correctly as nothing at all. Very probably the alternative form of the question was instrumental in directing the children's thoughts away from the possibility of the figure being barefoot, and in leading them to pronounce in favour of either sandals or boots.

There was an interesting difference in the replies to the two consecutive questions, "Was there some blue ribbon round the cat's neck?" and "Was there a bell on the ribbon?" The former was much more suggestive than the latter, which again may be due in part to the wording of the question. Since blue ribbon was mentioned, it tended to suggest that there was actually ribbon present but that the only doubtful point was its colour. The omission of the word 'blue' from the question would probably decrease its suggestiveness.

The least suggestive questions are those which refer to the most prominent components of the event or to those parts which are uncommon or irrelevant. Hence a question such as: "Had A a white waistcoat?" was answered correctly every time by the normal children, but 11 of the defectives replied in the affirmative. Thus, under normal conditions, it seems probable that a white waistcoat would have been noticed by the normal children. Again, when asked if A had a moustache, the normal children answer correctly in the negative on every occasion, but 15 of the defectives give affirmative replies.

Correlations. Coefficients of correlation¹ between the order of suggestiveness of the questions for the different groups tested were evaluated, and found to be as follows:

Groups	<i>r</i>
I and II	.48
I and III	.64
I and IV	.87
II and III	.87
II and IV	.73
III and IV	.79

¹ Evaluated by means of the product-moment formula.

Thus there is a high, positive correlation between the order of suggestibility of the different questions for the different groups, showing that the suggestiveness of each question remains fairly constant from group to group.

In connexion with Groups I and II, the children were classified in the order of their general intelligence, and these intelligence classifications were correlated with (*a*) range of narrative, and (*b*) number correct in interrogatory. The coefficients of correlation between the order of intelligence and the range of narrative were .45 and .16 for Groups I and II respectively. Since the children of Group I varied in age from 11 to 14, whilst those of Group II were of approximately the same age, it seemed possible that the difference in the values obtained for Groups I and II may have been due to the influence of this age-factor.

Accordingly, in the case of Group I, coefficients of correlation were obtained between the age of the children and the range¹ of the narrative, and also between the former and their order of intelligence. These were found to be .32 and .46 respectively. By means of the method of partial correlation it was now possible to find the closeness of the relationship between the range of the narrative and the intelligence classification independently of their common relationship to the age of the children. This value worked out at .36. When 'intelligence' was made constant, the correlation between age and range of narrative fell from .32 to .14. Thus there is a tendency for the more intelligent children to give the longer narrative, and this tendency is slightly more dependent upon the intelligence than upon the age of the children.

Coefficients of correlation were also worked out between the number correct and the number incorrect in the interrogatory, and were found to be as follow:

			I	II	III	IV
1st test21	.67	.27	-.57
2nd test11	.65	.18	-.12

These values show that there is, on the whole, a slight relation between the number of correct and incorrect replies in the case of the normal children (Groups I and II). The number of incorrect replies is roughly proportional to the number of correct replies. This uniformity, however, does not apply to the answers of the defective children. Hence it seems that the individual fluctuations are greater in a group

¹ In the narrative the accuracy is so great that marks given for it are practically identical with those given for 'range.' The above correlations therefore may be taken as representing the relation of either 'range' or 'accuracy' to the age and intelligence of the children.

of defective children than in a group of normal children. It will be noticed that the steadiness and uniformity of the girls of Group II furnish further evidence in this connexion.

'Reconstruction' of the Event.

The event may be 'reconstructed' from the replies to the questions which constitute the interrogatory, since these questions refer to all the necessary components of the event. The answers to each question are usually varied, hence the reconstruction is made from the 'majority replies,' that is, from those replies which occur most frequently in connexion with any particular item. In the present case, reconstructions have been made from the replies of the children of Group II (normal) and from those of Group V (defective). The incorrect parts are printed in italics, and the numbers in brackets under each part represent the extent of the correctness or incorrectness as a percentage of the total number of answers given to that question.

GROUP II (*normal*).

A lady and gentleman entered the room. *The gentleman was carrying a bag in his left hand, a hat in his right hand, and a stick* (57) (31)
 over his left arm. He had on a fawn coat, *a blue tie*, and was wearing (54) (78) (60)
 a black or white muffler. His clothes were blue, and his hat was of the (66) (36) (89)
 bowler type. He placed his hat on the piano and shook hands with (56) (82)
 the teacher. He had light hair, which was parted at the side. *He had* (57) (90)
a ring on his finger, and his watch-chain was made of gold. The colour (75) (93)
of his stick was brown, the shape of the handle was crooked, and a silver (67) (96)
band encircled the stick. A white handkerchief was to be seen in the (90) (88)
gentleman's over-coat pocket. The length of the bag was 19 ins., the
 breadth one foot, and the height 10 ins. This he placed on the desk. (68)
 The lady was wearing a blue dress, *a white blouse* with lace over it, and (71) (55) (63)
a black hat, one side of which was turned up *and which contained a* (59) (66) (33)

¹ Following Dauber's procedure (*op. cit.*).

black feather. She carried a fur which was unfastened, a *black muff*,
 (73) (100) (72)
a black bag and brown gloves. On the lady's muff were some flowers,
 (70) (70) (62)
 she took these off, held them for a short time, and then replaced them.
 (85) (82)
 The lady had black hair, was wearing a rose, and *was a little taller than*
 (62) (52) (53)
the gentleman.

On the bag were some letters, these were *M.P.L.*, and *on the left of*
 (81)
the bag was the letter M. The label on the bag was white, and the bag
 (44) (56) (100)
 was brown. *The first thing that the gentleman pulled out of the bag was*
 (37)
a statuette, the second a statuette, the third flowers, the fourth a picture,
 (53) (18) (30)
the fifth a picture, the sixth a picture, the seventh a picture, the eighth a
 (34) (30) (30) (25)
flag, the ninth a picture, and the tenth a picture. They were in the
 (80) (82)
 room for *eleven minutes.* The length of the picture was 31 ins., the
 width 22 ins., and it had no border round the edge. The picture showed
 (67)
 a cat sitting watching a canary in a cage. The breast, paws, and face
 (93) (75)
 of the cat were white, the back was grey, and *only two of the paws were*
 (38) (73)
visible. The title of the picture was "The Cat and the Canary," and
 (95)
the letters composing the title were yellow. On the picture were also some
 (38) (62)
red flowers on a table. The statuette represented a girl holding a basket.
 (42)
The colour of the girl's hair was golden, her cloak was white with a
 (53) (53)
yellow lining. She was also wearing sandals.
 (73) (80)

The gentleman pulled out a bunch of roses, three in number, *with a*
 (46) (45)
yellow flower in the middle. The book which he pulled out was red,
 (33) (93)
 and measured 8·9 ins. by 5·7 ins. The flag was 7·2 ins. by 4·7 ins., and
 had on it a yellow cross *on a yellow background.* The paper was the
 (87) (32) (46)
 'Citizen,' and was not opened. The pipe was brown, *curved*, and had
 (83) (69) (35)
 a black mouthpiece.
 (43)

GROUP V (*defectives*).

A lady and gentleman entered the room. The gentleman was carrying a bag in his right hand, a hat in his left hand, and a stick
 (48) (32)
 over his left arm. *He had on a black coat, a blue tie, and was wearing*
 (50) (30) (35) (65)
a white muffler which he kept on whilst in the room. *His clothes were*
 (93) (34)
black and his hat was of the bowler type. *He placed his hat on the*
 (65) (39)
piano and shook hands with the teacher. *His hair was black* and was
 (64) (38)
 parted at the side *and on his cheek was a cut.* *He wore eyeglasses and*
 (93) (87) (38)
had a ring on his finger, and his watch-chain was made of silver. *The*
 (84) (54)
colour of his stick was brown, the shape of the handle was round, *and on*
 (68) (85)
the stick was a silver band. *A white handkerchief was to be seen in the*
 (41) (88)
gentleman's overcoat pocket, and there was a flower in his coat.
 (65)

The lady was wearing a blue dress, a *white blouse*, with lace over it,
 (67) (75) (67)
and a blue hat, one side of which was turned up, *and which contained a*
 (50) (55) (71)
black feather. She carried a fur which was unfastened, a muff, gloves,
 (82) (64) (83) (95)
but no bag. *There was a dog's head on the muff, she took nothing off the*
 (50) (52) (68)
muff, and she replaced what she took off. The colour of the lady's hair
 (57) (48)
 was black, *her fur and muff were black and her bag was black, her gloves*
 (66) (65)
were also black. She was wearing a rose, *she carried an umbrella,* shook
 (50) (81) (55)
 hands with the teacher, *and put some roses on the piano.* *She afterwards*
 (52) (40)
picked up the flowers. *She also picked up a book and put it on the piano.*
 (28) (60)
She had a watch on her wrist.
 (83)

On the bag, which was brown, *were the letters L.M.R. and on the*
 (93)
right of the bag was the letter M. *There was also a white label on the*
 (53) (45)

bag. The gentleman pulled out of the bag first a statuette, second a
 statuette, third flowers, fourth a picture, fifth flowers, sixth a knife, seventh
 a book, eighth a picture, ninth a picture, and tenth a picture. The
 picture had a black border round the edge, and showed a cat sitting
 looking at a bird. The cat had a white breast, face, and paws, a black
 back, and part of the back was yellow. Only two of the paws were visible.
 The title of the picture was 'The Two Friends,' and the letters which
 composed the title were black. The bird was yellow and was still. At
 the side of the picture were some white flowers, there were also some
 red flowers on the table. There was some blue ribbon around the cat's
 neck, on which was a bell. The owner of the cat was looking through
 the window. The statuette represented a girl with brown hair, and she
 was wearing a white cloak with a white lining. She also wore boots, and
 held a baby in her arms. The flowers which the gentleman took out of
 the bag were red, 5 in number, and there was also a blue flower in the
 bunch. The book was red and contained a white paper. The flag was
 red and had on it a yellow cross; the flagpole was made of wood. The
 paper was called the 'Echo,' and was not opened.

VII. CONCLUSIONS.

The results of these experiments, when the findings of previous investigators are taken into account, support the following conclusions:

1. As a rule, the evidence of children is reliable only when it is given spontaneously. In such a case it is decidedly valuable and worthy of the utmost consideration.

The reports of the normal children are distinctly superior, both qualitatively and quantitatively, to those of the mentally defectives.

2. The narrative is chiefly devoted to the description of actions, and the enumeration of items and positions of objects. Other categories are relatively ignored. The interest of the children in movement illustrates very clearly one of the defects of a picture as material for a testimony experiment. Colours, sizes, and shapes are mentioned with greater frequency by normal than by mentally defective children.

3. The interrogated evidence is much less reliable than the spontaneous report. More than one-third of the replies of the normal children and more than one-half of those of the mentally defective are incorrect. Only those questions which relate to the chief and outstanding features of the event are answered with a high degree of accuracy. As in the narrative, the most reliable sections of the interrogatory are those which refer to actions and items.

4. A repetition of the questions after an interval of seven weeks showed, contrary to expectation, that this period of time had little effect on the children's memory of the event. The accuracy of the second series of replies was only slightly less than that of the first.

5. The knowledge of past experiences of similar situations has a considerable influence upon the replies given. Components of the event which are vaguely perceived or not perceived at all, are often interpreted or supplied according to the mode in which they are usually experienced. The relevancy of the situation and the range of the child's experience also determine in part the nature of the replies given.

6. Children's evidence bearing on the colours of objects is very unreliable. Usually colours which are not very prominent are very inaccurately described. Whenever there is any uncertainty about the existence of an object, the uncertainty is accentuated in the responses to the question about its colour. The mentally defective children err to a greater extent than the normal children in this respect.

7. In every case the duration of the event was enormously over-estimated; there seems to be a definite connexion between the number of details observed and the amount of the over-estimation, corresponding to the familiar illusion of the estimation of 'filled' intervals.

8. Most of the children are susceptible to suggestion, the susceptibility increasing as the interval between the event and the process of recall becomes greater. There is no correlation between general intelligence and suggestibility, but there is a small correlation between resistance to suggestion and the age of the subjects.

The most suggestive questions are those which refer to the less important or more obscure features of the event, and especially to those

suggested components which might be expected to exist. On the other hand, those questions are least suggestive which refer to the most prominent, uncommon, or irrelevant components of the event.

The order of suggestiveness of the questions is approximately the same for each of the groups tested.

9. There is no evidence to show that the testimony of normal children of the age tested is affected by sex differences, although that of the mentally defectives shows signs of being influenced by this factor.

10. The methods of teaching adopted in different schools for normal children may exert a considerable effect on the testimony of the children in these schools. In the school in which 'free discipline' was a prominent feature the individual differences between the children's performances were more marked than in the school in which the discipline was more rigid.

11. The testimony of the mentally defective differs from that of the normal children in the following respects:

(a) The narratives of the mentally defective children are fragmentary and disconnected, the event is described independently of its chronological order. The range is shorter and the deviations from the actual event are greater than in the case of the normal children. The evidence upon the categories of colours, shapes and sizes is very meagre.

(b) In the interrogatory, the number of incorrect replies given by the defective children exceeds the number of correct answers. The reverse obtains in the case of the normal children. The replies of the defectives are less accurate and more varied than those of the normal children; the level of accuracy is lower throughout all the categories.

(c) The defective children generally have no conception of the absolute magnitude of objects, and their attempts to estimate the duration of the event are equally futile.

(d) They are less able to resist suggestion than normal children.

(e) The defective children cannot resist the temptation to answer every question. It is a source of weakness in their evidence, and in this respect generally differentiates them from the normal children.

(f) The environment and past experience of the defective children have a greater influence upon their evidence than is the case with the normal children.

(g) In all the categories the level of accuracy attained by the mentally defectives is much lower than that of the normal children.

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THE CONDITIONS WHICH AROUSE MENTAL IMAGES IN THOUGHT¹.

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I.

- § 1. *The nature of the experiments.*
- § 2. *The method.*

II.

- § 1. *Analysis of the mathematical group.*
- § 2. *The results.*
- § 3. *Analysis of the historical group.*
- § 4. *The results.*
- § 5. *Analysis of the grammatical group.*
- § 6. *The results.*

III.

General conclusions.

I.

§ 1. THE original purpose of the following experiments was to demonstrate to the subjects who took part in them the importance of imageless thought, and to show them by their own introspection the distinction between a mental image and the meaning of which the image is merely the vehicle. The subjects were told that they were to investigate the existence and importance of thought without images, and to try to find out the content of such thinking. They were also told to try to distinguish, as far as they could, between the thinking act and the thought.

There were fifteen subjects, eleven men and four women, and by a preliminary experiment it was found that they were all possessed of fairly strong mental imagery. Four were strongly visile, two strongly

¹ Read before Section I (Subsection of Psychology), at the Meeting of the British Association for the Advancement of Science, Birmingham, 1913.

audile, one strongly motile. The others belonged to no single predominant type but had strong imagery of a mixed kind; three being auditory-visiles, three auditory-motiles and two motor-visiles. It is interesting to observe that the four women belonged to the motor or auditory types and were poor visualisers. One was strongly motile, one was strongly audile and two were motor-audiles. With four exceptions the subjects were post-graduates, and all had previous practice in introspection in connexion with some earlier experiments, so that they all knew what was required of them.

§ 2. Twelve statements were selected, four involving mathematical conceptions, four historical, three grammatical, and one was a couple of lines from Milton. With ten of the statements the fifteen subjects were tested, with the remaining two only thirteen.

The subjects were told to record on a sheet of paper everything they could discover by introspection after each statement had been read twice by the experimenter. They were told to put down everything, however unimportant it appeared to them, *e.g.* if they were conscious that during the act of thought they felt muscular strain or tension, or if they were conscious that their minds took certain 'directions.' In fact all details, whether mental or physical, were to be noted. They were also told to put down first of all whether they realised the meaning of the statement read to them; and as soon as the meaning was realised the process of thought which had led to its realisation. If possible they were to state what the realisation consisted of; and whether it involved mental images or not. The fact of agreement or disagreement with the statement was said by the experimenter to be immaterial; the important thing was the realisation of the significance of the statement and the imagery, if any, aroused. In cases where images did arise they were instructed to state whether the realisation of meaning preceded or succeeded the occurrence of the mental image.

These instructions were only fully carried out as regards that part of them which related to the realisation of meaning and to the occurrence of images. An examination of the records revealed, quite unconsciously on the part of the subjects, the conditions under which mental imagery arises. The subjects stated definitely in several instances why the images came and what part they played in enabling them to realise the meaning. They had no preliminary theory on the subject, but simply recorded their introspective observations.

The statements were not dictated in the order in which they are here analysed; to secure variety and freedom of thought and to maintain

422 *Conditions which arouse Mental Images in Thought*

interest in the experiment, the statements belonging to different groups were interspersed, so that, as a rule, two of a like kind did not come together.

II.

§ 1. The propositions in the first group were :

i. (a). *The whole is greater than the part.*

i. (b). *The whole is equal to the sum of its parts.*

i. (c). *If equals are added to equals the wholes are equal.*

i. (d). *If unequals are added to unequals the wholes are unequal.*

The following is an analysis of the results obtained after classifying the introspective records :

i. (a). Four subjects realised the meaning before the phrase was finished, three without any mental image.

Two recognised the phrase first, and the meaning later. Of these one had verbal images ; the other no image, but a 'sense of its truth.'

Four realised the meaning first, and images appeared in three cases after realisation.

Five subjects realised the meaning by the aid of visual images.

Thus five subjects obtained no images at all during the thought process ; and in three other cases the images which came played no part in securing assent to the proposition, as they appeared afterwards. Of those who obtained images some got pictures of geometry books used at school, or of divided circles, or of solid objects such as spheres divided up. The phrase had for some subjects obviously acquired a meaning which was reinstated in consciousness immediately one began to utter it.

i. (b). Five subjects realised the meaning without images, but two of them obtained images later. One of the five disagreed with the proposition on the ground that it was not true of some conceptions, *e.g.* the soul ; another obtained the meaning thus, "I conceived the whole as having parts, and if so the sum of the parts must give the whole." Both these subjects are students of moral science.

Seven subjects realised the meaning by the help of visual images of circles, squares, triangles or solids divided up.

Of the remaining three subjects, one obtained an image of the series $1 + \frac{1}{2} + \frac{1}{4} + \dots \infty = 2$, then an image of a dissected disc put together ; another obtained a very clear verbal image which was "so strong at first that I did not grasp the significance of the statement, then I assented immediately" ; the third realised the significance before the

completion of the sentence and had an image of a solid figure. Another subject, too, realised the meaning in anticipation at the word 'sum.'

Thus only three subjects failed to receive any image during the whole process.

i. (c). In this case nine subjects realised the meaning immediately without the help of mental images. Four others realised it by the help of verbal images, and the remaining two by the help of images of geometrical figures. Both these last are very slow in grasping ideas.

Four subjects agreed to the statement in anticipation before it was finished. Several others said that assent was instantaneous. The proposition, owing to its occurrence in geometry books, had for them an acquired meaning which was realised immediately.

i. (d). This statement is obviously not necessarily true, and was introduced in order to see by what means the falsity was realised.

Only two subjects realised the falsity immediately, and neither of them used imagery for the purpose.

Six subjects suspended judgment before realising that it was false; the realisation taking place in all cases by means of images.

The remaining seven made use of images.

Of these last, two disagreed with the proposition and used visual images; in one case these were very clear and vivid.

Two had doubts about its validity, but on the whole thought it was true; these had images.

Two were very hazy about the meaning and obtained vague images; one of them said that "the possible meaning was clear without definite images." Apparently, then, the images that he got later were the result of deliberation.

One had a visual image of lines in a book. He had agreed to the proposition at first without using images and then the possibility of its falsity occurred to him and the visual image came. This case is especially noteworthy as this subject had not received images in any of the three former cases.

The important feature of experiment i. (d) lies in the fact that only two subjects failed to get images, and these were the only cases in which the falsity of the proposition was realised at once.

§ 2. Several conclusions emerge as the result of the foregoing analyses of the subjects' introspection. In the first place it is perfectly clear that a considerable amount of thinking is entirely independent of mental images. Of the 60 thought-processes of the 15 different subjects, 24 or 40 % occurred without mental imagery. As the

424 *Conditions which arouse Mental Images in Thought*

statements selected would involve mental images of a very simple and definite type, if they occurred, namely of simple geometrical figures; and as the frequent mention of images during the experiment would in itself act as a suggestion to arouse images which would otherwise not occur, it seems probable that under normal conditions of thinking images would not arise in more than 50 % of the cases¹.

In some cases strong imagery interferes with the act of thinking; thus one subject in i. (b) said that "the image was so strong at first that I did not grasp the significance of the statement." But in others, especially where there is some difficulty, a strong image may aid the thought-process; in i. (d) one subject had "a picture of clusters of triangles and other geometrical figures, and decided that nothing whatever could be stated of them." The image in this case obviously plays the same part as a diagram does in a geometrical proof.

If we compare the statement i. (a) with i. (b), the former involves a simpler conception, being easier consequently to grasp, and the experiment shows that images were less frequent in i. (a) than in i. (b). This result seems to be general. Images tend to appear if the realisation of meaning is not at once clear, or if there is a delay or a struggle in consciousness. Where the meaning is easily grasped or where assent has been previously given there seems to be no tendency to embody the thought in an image. Thought is, in these cases, carried on by meanings. If the meaning is very clear to the subject the image, as we have seen, may actually interfere with thought, since the subject tends to dwell on the image to the exclusion of the meaning. These results are made clearer by a comparison of i. (c) with i. (d), the former being much easier to realise. In the case of i. (c) nine subjects grasped the meaning without the aid of images, whereas in i. (d) only two subjects failed to receive images. The increased difficulty of realising the meaning resulted in thirteen subjects receiving images of some kind.

Again, suspension of judgment and doubt, both of which may be regarded as instances of delay or struggle in consciousness, are conditions which facilitate the emergence of mental images, as i. (d) clearly shows.

The following introspection results bear out these conclusions. With reference to the proposition, "If equals are added to equals the wholes are equal," one subject said, "Of course they are; assent came long before there was any thought of criticising (examining?) the statement." Another said, "This was very easy and needed no consideration; beyond

¹ Out of the 176 cases examined 77 obtained the meaning without images.

suggesting thoughts of arithmetic and geometry the statement was accepted at once." There were no images here according to the subject. A third said that she assented at once; and anticipated the meaning before the whole was read. The realisation of meaning was instantaneous and there were no images. The proposition i. (d), namely, "If unequals are added to unequals, etc.," was taken before i. (c), i.e. "If equals are added, etc.," and the introspective results of one subject are sufficiently instructive to be quoted in full. With reference to i. (d) he said, "Doesn't sound probable somehow. Rather a wrench necessary to enable me to realise that you have two sets of things to work with. Then I saw that it is a lie (*sic*) as a picture of heaps of plain wooden bricks arose, thus:



At the statement i. (c) he said, "Sounds much more reasonable. Of course it's true. No need to fetch the bricks out to prove that one (i.e. no image at all)."

§ 3. The propositions in the second group were:

- ii. (a). *Whilst Britain was prospering under Roman rule the Roman Empire itself was beginning to show signs of decay.*
- ii. (b). *Every historical event has a political cause.*
- ii. (c). *The whole organization of society was once based upon the system known as feudalism.*
- ii. (d). *Mechanical inventions have had important effects upon the social life of England.*

An examination of the results of introspection yielded the following analysis:

ii. (a). Four subjects had either very faint images or none at all. One of these said that he deliberately concentrated his attention on the meaning of what was uttered. This latter case is noteworthy, as the subject in the first series of experiments was very rich in images. Two of them found the statement easy to follow and assented readily.

Eleven subjects had visual images of maps, soldiers, school pictures, Caesar's landing in Britain, etc. In seven of these cases there were distinct indications of a conflict or movement backwards and forwards in thought.

ii. (b). Seven subjects received no images. Of these, two assented to the proposition immediately; but three others had difficulty in grasping

426 *Conditions which arouse Mental Images in Thought*

the meaning. Two of these latter three are moral science students who are practised in thinking in abstract terms and the third has had some philosophical training. This perhaps explains the unexpected result, as these students are accustomed to the terms 'event' and 'cause' and have acquired the habit of thinking of their meaning apart from images.

Seven subjects had images, some very faint. Four of these found difficulty in deciding on the meaning; one said explicitly that the image was called up for this reason.

The remaining subject felt inclined at first to call up visual images of historical events to test the truth of the statement, but combated this by concentrating his attention on the bare meaning instead.

ii. (c). (13 subjects.) Four subjects had no images. One of them disagreed at once and one decided to accept the statement at once.

Nine subjects received images; three of these were verbal images and two were images of history books. One subject had to dispel the image before he could seize the meaning. Five of them agreed with the statement immediately.

ii. (d). (13 subjects.) In reading this sentence a distinct pause was made after the words 'mechanical inventions.'

Three subjects had no images and in each case assent to the proposition was immediate, without effort.

Ten subjects had images, nine visual and one auditory. Two of these stated that the images were due to the pause in reading; and in fact the majority of the images were directly concerned with some form of mechanical inventions. One of the subjects had a coloured verbal image of the word 'social'—light to dark yellow varied with green.

§ 4. Examination of the introspection records of this second group reveals some further conditions which facilitate the occurrence of mental imagery, and also confirms the conclusions previously reached.

Immediate or ready assent to a proposition, or ease in understanding, both of which imply the free flow of thought, usually means that the subject has no images in the focus of consciousness. But there are exceptions to this which need further inquiry, for in ii. (c) five subjects, who agreed to the statement readily, had images. If a subject makes a deliberate attempt to concentrate his attention on the meaning of a statement he may succeed in suppressing images which would otherwise occur.

In cases of conflict, or disagreement with a suggested statement, which manifestly implies a conflict of some sort, images tend to appear. To make use of a bold metaphor, the mental image seems to be the

result of friction in thinking just as a spark may arise from the friction of two hard bodies. This is shown very clearly in some of the introspection records with reference to ii. (b). One of the subjects was struck by the difficulty of defining 'historical event' and he was aware of images of Pilgrim Fathers, Fire of London, Balkan War, etc. He could not assent to the proposition after considering these cases. As a rule this subject does not receive images. Another subject said "Such a statement cannot be accepted without investigation. *Therefore one draws images.*" He proceeded to get images of the Spanish Armada, the English Revolution and Voyages of Discovery. Then having fully realised the meaning of the statement he gave a qualified assent. A third subject said that she could not understand the meaning of the proposition although she had images, and then proceeded thus: "Further attempts to make a meaning brought up a picture of someone speaking in Parliament." Here the attempt to overcome a difficulty is assigned as a reason for the development of the image. Another example of the same process was given by a particularly careful subject who is very conscientious in his statements, thus: "First impulse to be inclined to agree. Then it flashed across me that there were cases in which it was not true. For a moment or two I seemed to have an example of this near at hand and strove in vain to find it. Then at last it flashed across my mind as a faint picture." In this case we actually observe the birth of the image in a presented difficulty that requires to be solved.

A further condition favourable to the arousal of mental images was revealed by the ii. (d) series. Emphasis or a pause which constitutes a break in the free flow of thinking is favourable to the production of imagery. Thus one subject said "The pause after 'mechanical inventions' gave opportunity for a faint image of an engine." Another stated that "After 'mechanical inventions' the hesitation gave time to picture such." This condition, too, seems to fit in with the general idea of a conflict or struggle which is again shown to be favourable to the development of imagery.

It was shown above that in certain cases a strong image may obstruct the attempt to understand. Another instance of this is provided by the experiment ii. (c). The painstaking subject recently referred to said "Image of society under the shape of a mob. Image of feudalism as a picture of a man doing allegiance to his liege lord. When I tried to realise the significance of the statement it was twice obstructed; at first by the picture of my old history room at school, then by my history book open at the page on feudalism. These images being dispelled the

428 *Conditions which arouse Mental Images in Thought*

thing was then first clearly taken in, though I think I had grasped it faintly on hearing it for the first time."

It seems, then, that a mental image is due to an obstruction in the free flow of ideas, so that for a thinking process to proceed to its proper conclusion the attention must be concentrated on the meaning to the exclusion of the image.

§ 5. The dictated statements in the third group were as follow :

iii. (a). *All verbs that make a statement must be accompanied by some noun.*

iii. (b). *Grammar is useless because we speak well without a knowledge of it.*

iii. (c). *You should never use a preposition to end a sentence with.*

iii. (d). *Laughing to teach the truth*

What hinders? As some teachers give to boys

Junkets and knacks, that they may learn apace.

After classifying the results of introspection the following analysis was obtained :

iii. (a). Six subjects received no images. Four of them realised the meaning promptly and another deliberately concentrated his attention on the meaning, another assented to the statement slowly.

Six subjects had verbal images. In *all* these cases the realisation of the meaning was slow and laboured. In one case there was a conflict in the subject's mind, in another a doubt which was settled by assent without conviction.

Two subjects obtained visual images. In one case there was a conflict and in the other a distinct search for examples.

One subject made no statement about images.

iii. (b). Eleven subjects obtained no images during the realisation of the meaning of the statement, though in some cases images came later. In *all* these cases the realisation was easy and in several it prompted a train of reasoning.

Four subjects received images. One of these did not realise the meaning of the statement at once, but another did. One subject had an image of his college where this question was discussed.

iii. (c). Eight subjects realised the meaning without any images at first, but two obtained images afterwards. Five of them realised it easily and immediately or by having heard it previously; in one case the realisation was slow.

Seven subjects received images. Of these, one saw a sentence having the preposition 'with' clearly defined and another heard this word by

an auditory image. Three of these subjects realised the rule first and saw the absurdity of it later, whilst one apparently did not see the absurdity of the rule.

iii. (d). The results of this series require a more detailed analysis.

Six subjects obtained no image at all; of these four understood the meaning of the lines promptly and thoroughly, and two realised the meaning slowly, in one case not thoroughly.

Three subjects obtained what may be described as an *associative image*, namely an image not directly called up by the lines but evoked by association with their meaning. In these cases the image was that of a book on education in which a similar doctrine to that expressed in the lines was discussed. Now those who had these images must have realised the meaning before the images came, since such images depend on understanding the meaning. We may therefore say that nine subjects realised the significance of the passage without the aid of images; seven promptly and two slowly¹.

Six subjects obtained images before realising the meaning. Four of them were visual images, one was verbal and one was a taste image of junkets. In two of these subjects the realisation was slow, two did not understand the passage, but the remaining two realised its meaning immediately.

§ 6. The results obtained from group iii. thus serve to confirm the previous conclusions; *e.g.* in iii. (d) we see that, on the whole, prompt and thorough understanding coincides with the absence of images. The passage was not very easy to follow at first and directly suggested several images, but apparently those who concentrated their attention on the meaning failed to get these images. On the other hand those who did get images, as a rule realised the meaning slowly or not at all. This is verified also by considering the results obtained from series iii. (a). Similarly with regard to iii. (c), of the seven subjects who understood the rule immediately or easily five obtained no images. Series iii. (b) in this group of cases also shows that where the reasoning to a conclusion takes place easily images, as a rule, do not occur.

¹ G. H. Betts in his admirable study of *The Distribution and Function of Mental Imagery* (New York, 1909) says "our associative machinery may bring before the mind many elements which have no function in the thought of the moment, but are only incidents, by-products of the thought process" (p. 49).

III.

The question which the experiments have helped us to solve is concerned with the part played by mental imagery in thought proper as opposed to reverie or day dreaming. In the latter case images come and go and we can no more explain the origin of the particular images that arise than we can account for the things we see when our eyes are open. Where, however, there is a definite end in view, a specific problem to be solved an answer seems possible. For this purpose it is necessary to draw a distinction between relevant and adventitious images. In a particular train of thought there may be a number of images present which neither aid nor hinder its development,—adventitious images. The conditions favourable to the awakening of relevant images, which form an integral part of the thought process, appear to be the same in the majority of cases. And these conditions have in many instances been unmistakably indicated by the subjects during the course of the experiment. Their evidence is made more trustworthy by reason of the fact that it was given spontaneously without any preconceived notions on the matter. None of the subjects had any idea before the experiment started of what the conditions were, nor were they consciously engaged in discovering the conditions, but in answering quite different questions.

The experiments show that any delay or conflict in consciousness is a favourable condition for arousing a relevant mental image, that is, one that will in some way tend to help towards a cessation of the conflict. All the other conditions which we have found to be suitable for stimulating the production of mental images are reducible to this general formula. Thus, conflict or disagreement with a suggested statement, an attempt to overcome the difficulty of understanding a proposition, suspension of judgment, doubt, emphasis or a pause, all have been shown to produce mental images abundantly. And all of these are examples of struggle or delay in thinking¹.

The experiments also show directly that the contrary set of conditions are unfavourable to the production of images. Thorough or immediate understanding, an easily grasped conception, ready assent to a proposition, straightforward or unimpeded reasoning, are all cases in which, as a general rule, images play no part. Further, concentration

¹ This agrees with the conclusion reached by Betts (*op. cit.* 94) that images tend to emerge "at points where our thinking is baffled," but his other conclusion that the images at these points are mostly irrelevant is doubtful.

of thought on meaning is unfavourable to the stimulation of mental imagery, but this cannot be brought easily under the above general formula.

Thus, whatever promotes the easy or unimpeded flow of thought is unfavourable to the production of mental imagery and *vice versa*. The law will perhaps be made clearer by comparing the stream of thought to the flow of electricity in a conductor; where the resistance is high, heat becomes apparent, and where it is sufficiently increased, light breaks out.

Possibly we have here an explanation of the fact that children have richer and more vivid imagery than adults. For difficulty in understanding abstract ideas and relative inability to fix attention on meanings are just the best conditions for arousing strong mental images¹.

¹ After this paper was completed my attention was called to a work by Dr Aveling *On the Consciousness of the Universal and the Individual* (Macmillan, 1912). Aveling correctly maintains that thinking can take place with concepts alone as contents; and on p. 170 he rightly states that "if we stop to ask ourselves what an unfamiliar word means, we generally discover that another word, or an image, is aroused as exemplificative of its meaning." This is but a particular case of our general law that a difficulty will produce an image.

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ON CHANGES IN THE SPATIAL THRESHOLD DURING A SITTING.

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1. *Object of the paper.*
2. *Description of the experiments.*
3. *The psychophysical methods used.*
4. *Catch errors.*
5. *Processes of calculation.*
6. *The raw results.*
7. *Corrections for personal differences and diurnal variation.*
8. *Probable errors and significance of the results.*
9. *Influence of the experimenter.*
10. *Summary.*

1. OBJECT OF THE PAPER.

IN some previous experiments¹ (carried out for another purpose), the writer was led to suspect that during one sitting there was a tendency for the spatial threshold at first to sink, and later to rise again. A sitting in those experiments lasted about twelve minutes, during which one hundred applications of the aesthesiometer were made to the right forearm of the subject. At that time six subjects were examined in all, but only in one case (subject No. 2, a man) could the suspected tendency be considered to be clearly proved. The present experiments have been carried out to test the point further, and have confirmed it to a reasonable degree of probability.

¹ G. H. Thomson, "Comparison of Psychophysical Methods," this *Journal*, 1912, v. 233.

2. DESCRIPTION OF THE EXPERIMENTS.

The experiments were carried out with the same simple apparatus and with the same precautions as were noted in the earlier series on subject 6¹. Some additional precautions, and certain changes in the psychophysical methods used, will be noted presently. Subject 6 was again available, and in addition three new subjects were examined, numbered 7, 8, and 9, of whom 7 and 9 were women. Subject 8 was the writer himself, and the experimenter in this case was No. 6. For subject 7 and also for subject 9 both No. 6 and No. 8 acted as experimenters, sometimes at alternate sittings, sometimes at alternate portions of a sitting. For subject 6, No. 8 acted as experimenter. A third person was always available as clerk.

Subjects 7 and 9 had no knowledge whatever of the purpose of the experiments, nor had subject 6 in the first series; but this subject had some suspicion of it in the second and third series. Of course the writer when acting as subject had to endeavour to avoid any bias which his foreknowledge would bring. The data suggest that the endeavour to be impartial resulted in going to the other extreme, for the expected result is absent in the case of subject 8. It may be however that this fact is due, not to the endeavour to avoid bias, but to the difference in sex; or there may be other reasons which will be suggested later.

No subject was told during a sitting whether the answers given were correct or not. At the end of a sitting however each subject except No. 7 was told how he or she had done, especially how many catch errors had been committed. No. 7 was given no information whatever.

During the experiments on this subject, and during most of the experiments on No. 6, the arrangements were such that not only the subject but also the experimenter was ignorant whether the answers were correct or incorrect. This was managed in the following way. The experimenter, who sat on one side of a screen through which the subject's arm projected, was provided with a list showing the order in which the different applications of the aesthesiometer were to be made; and after warning the subject that he was going to begin, he proceeded through this table at a regular rate fixed by the unobtrusive ticks of a clock. The aesthesiometer was thus always applied at the

¹ *Op. cit.* 214 etc.

434 *Changes in the Spatial Threshold during a Sitting*

same intervals and for the same length of time. The subject communicated her judgments *one*, *two*, or *doubtful* to the clerk by signs invisible to the experimenter, who was thus kept in entire ignorance of how the sitting was turning out. The writer has not however detected any differences between those series of experiments in which this practice was followed and those in which the judgments were communicated by word of mouth and were therefore heard by the experimenter.

3. THE PSYCHOPHYSICAL METHODS USED.

By the 'method' used is meant the type of sequence according to which the various stimuli were presented to the subject. The 'process' of calculation afterwards applied to the data is another and independent question which is discussed on a later page.

The method used in the case of subject 6 was for the sake of continuity the same as that employed in the previous experiments¹ where it is more fully described, viz. the Method of Non-Consecutive Groups. The distances used for the aesthesiometer for this subject varied by half-centimetres from five centimetres downwards, and were always constant. A group consisted of five applications of say three centimetres, mixed with five catches in which only one point was presented. The groups followed one another in a chance order which was varied in a cyclic manner from day to day and was altered *in toto* at the commencement of every new series of ten sittings. The sitting proper consisted of ten groups, and therefore of a hundred applications of the instrument. In the case of this subject only, each sitting proper was preceded by a preliminary group, always at three centimetres. The other three subjects however were given no such daily preliminary practice.

With subjects 7 and 9 a method was followed which was more suitable for the purpose here in view. This method may be defined, in terms of the nomenclature suggested elsewhere by the writer², as the Method of Right and Wrong Cases with Catches. In any one sitting five stimuli differing from one another by steps of one centimetre were used, in addition to the catch stimulus consisting of one point only, or zero distance. For example in a certain sitting the stimuli used might be

1, 2, 3, 4, 5 cms.

¹ *Op. cit.* 204, 214 ff.

² *Op. cit.* 204.

TABLE 1. *Method of Non-Consecutive Groups. Subject 6.*
February 24th, 1913, 8.21 a.m.

Pre-liminary 3 cms.		i 1 cm.		ii 4½ cms.		iii 2½ cms.		iv ½ cm.		v 1½ cms.		vi 4 cms.		vii 5 cms.		viii 2 cms.		ix 3½ cms.		x 3 cms.	
Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A
2	w	2	w	1		1		2	w	1		1		1		1		1		1	
1		1		2		1		1	w	2	w	1		2		2		1		2	
2	w	2	w	2		1		2	w	2	w	1		2		2	w	1		2	
2	w	1		1		2		1		1		1		2		2	w	2		2	
1		1		2		2	w	1		1		2		1		2	w	2		2	
1		2		2		2	w	2	w	2	w	1		2		2		1		1	
2	w	2	w	1		2		2	w	2	w	2		1		2	w	1		1	
1		1		1		1		2	w	1		2		1		1		2		1	
1		2	w	1		2		1		2	w	2		1		1		2		1	
2	w	1		2		1		1		1		2		2		1		1	w	2	

The Q (question) columns indicate whether a double or single touch was presented. The A (answer) columns give the answers; blank means correct, *w* means wrong. No answers *doubtful* were given at this sitting. There is one catch error, in period ix.

TABLE 2. *Method of Right and Wrong Cases with Catches. Subject 7.*
February 24th, 1913, 8.32 a.m.

i		ii		iii		iv		v		vi		vii		viii		ix		x	
Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A	Q	A
a	w	1		e	w	d		b	w	1		a	w	b	w	e		b	w
e	w	b	w	1		c		1		e		e	w	1		1		1	
1		e		1		1	w	a	w	1		d		c	w	1		d	
1		1		c	w	1		1		d		d	w	1		1		1	
1		a	w	a	w	e		e		c		1		d		c		1	
b	w	1		1		1		1		b	w	1		a	w	1		1	
c	w	1		1		1		d	w	1		1		e		1		a	w
1		c		1		c	w	c		a	w	c	w	1		1		c	
1		1		1		1		1		1		b	w	1		b	w	1	
d		d		d		1		1		1		1		1		a	w	e	

Here *a*, *b*, *c*, *d*, *e* mean double touches of 1, 2, 3, 4, 5 cms. respectively.

436 *Changes in the Spatial Threshold during a Sitting*

At the next sitting the values of these stimuli would be changed each by the same number of millimetres, say to

1.2, 2.2, 3.2, 4.2, 5.2 cms.

and in the course of ten sittings every millimetre would be used¹. The lower limit was not necessarily one centimetre but whatever it was the stimuli in ten sittings covered five centimetres, millimetre by millimetre. This was done for a purpose not germane to the present issue: nor need it have been mentioned here except for completeness and because otherwise some of the decimals in the results might have appeared to be arithmetically impossible. The hundred applications in a sitting were divided into ten periods which are designated throughout this paper by the Roman numerals i to x. In each period there were five catches and five double touches, namely one application of each of the five distances used at that sitting. The order in which the stimuli were presented was determined before the sitting by drawing cards. The periods, in the cases of subjects 7, 8 and 9, are not groups, for in a group the stimulus must be the same throughout: but in the case of subject 6 the periods happen to be also groups. The accompanying Tables 1 and 2 are examples of these two methods. They were filled up column by column, beginning at the top of the left-hand column.

With subject 8 the method followed was in most respects the same as that just described, but the stimuli, which in this case were not changed from sitting to sitting, but were the same throughout, were in steps of one and a half centimetres.

4. CATCH ERRORS.

Before describing the processes of calculation used, it should be explained that the catch applications of only one point were not used in any way in the calculations. They were there simply as a check, and the rule followed was that a sitting was rejected in which more than five catch errors (out of a possible 50) occurred. This number was chosen because when the catch errors did become more numerous than this they usually became very numerous. When a series of experiments is begun on a new subject catch errors are usually very frequent in the

¹ See F. M. Urban, "Die psychophysischen Massmethoden," *Archiv f. d. ges. Psychol.* 1909, xv. 295—6, or *The Application of Statistical Methods to Psychophysics*, Philadelphia, 1908, 54—5.

first few sittings and the subject is apt to be incredulous that he can so often mistake one point for two.

In the case of subject 6 three series of experiments, each of ten satisfactory sittings, were carried out. The first series took place in March, 1912, and in fourteen consecutive sittings the number of catch errors never once rose above five. The first four sittings were treated as practice sittings; the remaining ten sittings form the first series. In these ten sittings there were only fourteen catch errors.

The second series was begun on Jan. 20, 1913. Between that date and Feb. 6th seventeen sittings were held and seven were rejected for catch errors: namely on Jan. 20th with thirteen, on Jan. 21st with six, and on Jan. 23rd with seven (which dates come at the beginning) and on Jan. 30th with twelve, on Feb. 1st with nine, on Feb. 2nd with twenty, and on Feb. 3rd with ten (a period when the subject was indisposed). The ten satisfactory sittings contained, in all, seventeen catch errors and form the second series for this subject.

The third series for this subject began on Feb. 21st, 1913, and ended on March 3rd. No sittings were rejected, and the total number of catch errors was eighteen.

With subject 7 two series each of ten sittings and one half-series of five sittings were carried out. The sittings began on Jan. 20th, 1913, when there were ten catch errors. On Jan. 21st there were four, but this sitting also was counted as preliminary practice. After that, not a single sitting was rejected, and the total number of catch errors was only fifteen in twenty-five sittings.

With subject 8 at the first sitting on March 26th, 1913, there occurred fifteen and a half catch errors (the answer 'doubtful' is counted one-half), on March 27th there were ten, after which all sittings were satisfactory. The total number of catch errors in these was ten in ten sittings.

With subject 9 the number of catch errors on the first day, March 30th, 1913, was nine, next day nineteen and a half, next day twelve and a half, after which only one sitting was rejected, on May 12th, when there were five catch errors and a half. The number of catch errors in the ten satisfactory sittings was sixteen and a half.

In short, the only rejected sittings under the rule were practice sittings at the beginning of a series, four sittings when subject 6 was not well, and one sitting with subject 9.

From the records no connexion between catch errors and period of sitting could be proved.

5. PROCESSES OF CALCULATION.

The process used was the Limiting Process¹. In the case of subjects 7, 8, and 9 this process could be applied simply and directly to the data as follows. Take for example period i in Table 2, and consider the stimuli *a, b, c, d, e* in this order, that is from the smallest to the largest. The first correct answer² is met at *d*, therefore *d* (here 4 cms.) is taken as the just perceptible stimulus. Next consider the same five stimuli in the order *e, d, c, b, a*, that is from the largest to the smallest. The first incorrect answer² is at *e* or 5 cms. and this is taken as the just imperceptible stimulus. The average of these two, i.e. 4.5 cms. is taken as the threshold in period i. The same proceeding in period ii gives 4 cms. for the just perceptible stimulus and 3 cms. for the just imperceptible stimulus, and the threshold is accordingly taken as 3.5 cms.

This simple process could not be applied in the case of subject 6 for here the stimulus in a period was the same in one sitting. For example in Table 1 period iii is exclusively devoted to the distance $2\frac{1}{2}$ cm. and although the answers suggest that the threshold is here a little below $2\frac{1}{2}$ cms. they do not enable us to calculate its value. At the next sitting, however, period iii was devoted to $4\frac{1}{2}$ cms., and in turn each stimulus was presented, day by day, at this particular period of the sitting. The results for period iii for the ten sittings from Feb. 21st to Mar. 3rd, 1913, were for example as follow :

<i>r</i> in cms.....	$\frac{1}{2}$	1	$1\frac{1}{2}$	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4	$4\frac{1}{2}$	5
Correct answers	0	0	0	0	3	0	1	5	5	5

To these Urban's formula³ is applied. This, which may be described as a theoretical instead of a direct application of the Limiting Process, takes the threshold as the mean of *T* and *T'* where *T* is the just perceptible stimulus and is given by

$$T = \Sigma Pr,$$

while *T'* is the just imperceptible stimulus and is given by

$$T' = \Sigma P'r.$$

¹ See Thomson, *op. cit.* 210.

² Or the answer *doubtful*, if it is first met.

³ *V. op. cit.*

Here the r 's are the centimetres of the stimulus values. The P 's and P 's can be most easily explained by working out the threshold for the numbers quoted above. This is done in Table 3, where the first column gives the r 's in centimetres. The second, or p column, gives the proportion of correct answers at each r , and is obtained by dividing the above numbers by five. p is the probability of a correct answer being given at the corresponding r . On the other hand, q is the probability of an incorrect answer, and is obtained by subtracting p from unity: this gives the third column in the table. The next column contains the P 's. Take the P for 3.5 cms., namely 0.08. This is obtained by multiplying the q 's together from the top of the table down to 3.0 cms. (this gives 0.4) and then multiplying by the p at 3.5 cms. (0.4×0.2 gives 0.08). All the P 's are obtained in a similar way. They are the probabilities of the just perceptible stimulus occurring at the corresponding points. The P 's are obtained by a very similar process, but beginning at the bottom of the table and multiplying a number of p 's and one q . They are the probabilities of the just imperceptible stimulus occurring there. The products Pr and $P'r$ are then formed. Then

$$T = \Sigma Pr = 3.06,$$

$$T' = \Sigma P'r = 3.4,$$

$$\text{mean threshold} = 3.23 \text{ cms.}$$

In this way the thresholds for each period could also be found for this subject.

TABLE 3. *Example of use of Urban's Formula. Subject 6. Third Series. Period iii.*

r cms.	p	q	P	P'	Pr	$P'r$
0.5	0	1	0	0	—	—
1.0	0	1	0	0	—	—
1.5	0	1	0	0	—	—
2.0	0	1	0	0	—	—
2.5	0.6	0.4	0.6	0	1.5	—
3.0	0	1	0	0.2	—	0.6
3.5	0.2	0.8	0.08	0.8	0.28	2.8
4.0	1	0	0.32	0	1.28	—
4.5	1	0	0	0	—	—
5.0	1	0	0	0	—	—
—	—	—	—	—	3.06	3.4

$$\text{Threshold} = \frac{1}{2} (3.06 + 3.4) = 3.23 \text{ cms.}$$

6. THE RAW RESULTS.

The above direct process of calculation gives for subjects 7, 8, and 9 a value of the threshold for each period of each sitting. At the end of a series of ten sittings these were averaged; that is—all the period i thresholds of No. 7 were averaged, all the period ii thresholds, and so on. This average is obviously uncorrected for diurnal variations in the threshold, and gives greater weight to the results obtained on days when the limen was high. The indirect process (Urban's formula) used for subject 6 is open to the same objection. Nevertheless it seems advisable to give these uncorrected results in the first place, and this is done in Table 4. Each column of this table is therefore based on ten

TABLE 4. *Average Changes in the Spatial Threshold on right forearm during the progress of a sitting.*

		Subject no. 6			Subject no. 7			Subject no. 8	Subject no. 9	Weighted Mean
		cms.	cms.	cms.	cms.	cms.	cms.	cms.	cms.	cms.
Progress of Sitting	i	2.98	2.15	3.25	3.30	3.28	3.99	4.50	3.50	3.38
	ii	2.66	1.95	3.11	2.95	2.28	3.29	4.57	3.20	3.05
	iii	2.40	1.85	3.23	3.25	2.68	3.34	4.42	2.60	2.99
	iv	2.16	2.19	2.58	3.05	3.28	2.99	4.17	2.45	2.83
	v	1.84	1.59	3.16	3.40	3.28	2.84	4.80	2.85	2.95
	vi	2.01	1.72	2.29	2.95	3.08	3.09	3.97	2.95	2.74
	vii	1.95	2.05	2.61	3.35	3.68	3.59	4.95	3.20	3.14
	viii	2.06	2.05	3.06	2.45	3.88	3.34	4.12	3.05	2.94
	ix	2.52	1.92	2.68	3.05	3.68	3.44	3.75	3.15	2.98
	x	2.06	1.85	2.15	2.65	3.28	3.44	4.45	3.05	2.84
Means ...		2.264	1.932	2.812	3.040	3.240	3.335	4.370	3.000	

sittings each of a hundred judgments, except column 5, based on five sittings only. Each column is divided into ten periods shown by Roman numerals. The first figure, for example, 2.98 cms., is calculated from a hundred¹ judgments, namely the first ten judgments at each of ten sittings. For subject 6 only, the sitting proper was preceded by a preliminary group of ten judgments. This group is not used here or elsewhere in this paper.

¹ But half of these were catches and are not actually used in the calculations.

The last column in Table 4 gives the weighted means of each row. The weighting is necessary because column 5 is based on five sittings only instead of ten. The first figure, 3.38, in the last column is, for example, obtained by adding together 2.98, 2.15, 3.25, 3.30, 1.64, 3.99, 4.50, 3.50, and dividing by $7\frac{1}{2}$. Obviously such a process is uncorrected for the personal differences between the subjects and gives undue influence to subject 8.

This last column is therefore the raw result of the experiments, uncorrected for diurnal or personal variations. Each of the ten figures in the column is based upon 750¹ separate judgments, or 7500 judgments in all.

It will be seen that the threshold is highest in period i and lowest in period vi. It falls sharply at first, then more slowly, then rises again and becomes irregular, but shows a sharp fall at the very end. If the individual columns are examined it will be seen that period i is with one exception always bigger than periods ii and iii, and with few exceptions is bigger than periods ii to vi inclusive.

It remains to be seen, in the following paragraphs, whether the correction for diurnal and personal variation makes any important change in this result. It will be found that this is not the case. Further it has yet to be shown that these results are significant, that is that the number of experiments is sufficiently large for the law of averages to eliminate 'chance' variations, *i.e.* those not due merely to the progress of the sitting.

7. CORRECTIONS FOR PERSONAL DIFFERENCES AND DIURNAL VARIATION.

A correction for personal variation from subject to subject can be immediately and simply applied, by dividing each number in Table 4 by the mean value of its column, and then averaging the rows afresh to get the weighted means of the ratios thus formed. This gives column A in Table 5. In variation with period it does not differ in any of its characteristics from the last column in Table 4.

In the case of subjects 7, 8 and 9 a more thoroughgoing correction is to divide each individual threshold for each period of each sitting by the average limen of that sitting. This eliminates both diurnal and personal variation at once. There are, it is true, certain mathematical objections to such a method, which are connected with the fact that the

¹ But half of these were catches and are not actually used in the calculations.

442 *Changes in the Spatial Threshold during a Sitting*

stimuli are necessarily in steps; but these objections are minimised by the daily change of stimuli in the case of subjects 7 and 9, and are not important. In the case of subject 6, however, where Urban's formula is used, it is difficult, if not impossible, to eliminate diurnal variation, and therefore only the above three subjects are referred to in columns *C* and *D* of Table 5. In column *C* the diurnal correction has not been applied; it is formed in the same way as column *A*. The differences between *A* and *C* are due only to the exclusion of subject 6. In column *D*, however, the diurnal correction has been applied, and comparison with column *C* shows that this correction has not been very important.

TABLE 5. *Result of correcting for Personal Differences and Diurnal Variation.*

		<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>
Progress of Sitting	i	1·143	0·022	1·108	1·115
	ii	1·029	0·028	0·983	1·008
	iii	1·003	0·022	0·969	0·958
	iv	0·953	0·023	0·928	0·932
	v	0·972	0·033	1·005	1·010
	vi	0·915	0·013	0·947	0·955
	vii	1·040	0·022	1·092	1·094
	viii	0·990	0·026	0·970	0·970
	ix	1·010	0·020	1·002	0·990
	x	0·944	0·022	0·987	0·965

- A. This column gives the weighted means of the ratios obtained by dividing each threshold in Table 4 by the average threshold of its series, given at the bottom of its column in Table 4.
- B. The probable errors of the numbers in column *A*.
- C. This column is formed in the same way as column *A* but is confined to subjects 7, 8 and 9 only.
- D. This column refers also to 7, 8 and 9 only, and shows the result of correcting for diurnal variation.

8. PROBABLE ERRORS AND SIGNIFICANCE OF THE RESULTS.

In Table 5, column *B* gives the probable errors of the numbers in column *A*. These probable errors are calculated in the following way.

It will be remembered that in order to eliminate personal differences each threshold in Table 4 was divided by the average value of its column. For example, the top row of Table 4, referring to period i of the sitting, then becomes

1·313, 1·113, 1·162, 1·086, 1·012, 1·195, 1·030, 1·167.

Of these numbers, 1.012 has only half weight. If this be borne in mind, their weighted mean will be found to be 1.143, the top number in column *A* of Table 5. If we now form the deviations of the above numbers from 1.143, square them, and perform the usual calculation for the probable error (keeping in mind the half weight of one member of the series), we obtain 0.022, the top number in column *B* of Table 5. The other numbers have been similarly obtained.

These probable errors enable us to say whether the differences between the periods are significant or accidental. They will be significant if the difference between two periods is of the order of six times the probable error of either, say about 0.14. Thus the difference in column *A* between period i and period iii is just significant, the difference between period i and period vi is more certainly so¹.

The above probable errors are calculated each from eight numbers which are themselves averages. It is perhaps more satisfactory to calculate them from the numerous individual thresholds, and this we can readily do for subjects 7, 8 and 9. With these three subjects there were altogether forty-five sittings, and therefore that number of measurements of the threshold for each period. Each of these is, as above, divided by the mean threshold for the day, to eliminate diurnal variation, and thus for each period there are forty-five ratios clustered round unity. From the distribution of these ratios probable errors can of course be calculated. For example, two of these distributions are :

Value of ratio3	.5	.7	.9	1.1	1.3	1.5	1.7
Period i.....	0	0	6	8	17	8	4	2
Period iv	2	0	8	18	11	5.5	0.5	0

By 17 values at 1.1 is meant that there are that number of values from 1.0 to 1.2. A value exactly at either of these points is split between the neighbouring compartments. The upper distribution corresponds to the average 1.115 at the head of column *D* in Table 5.

¹ This simple consideration is possible because the probable errors are all about the same size. It is visualised in a figure at the very end of this article, where the shaded area shows three times the probable error above and below the line representing the changing threshold. To be more accurate one ought to compare the differences with the probable error of the differences. For example, the difference between i and iii is 0.140 ± 0.031 , that between i and vi is 0.228 ± 0.026 . The latter is probably significant even if the most liberal allowance is made for the inaccuracy of probable error formulae for small numbers.

444 *Changes in the Spatial Threshold during a Sitting*

The semi-interquartile range is 0.13, and the probable error therefore approximately 0.02, agreeing well with the value in column *B* (which latter, however, includes subject 6 and refers to values uncorrected for diurnal variation).

The lower distribution corresponds to the average 0.932 in column *D*. Its semi-interquartile range is 0.125, and its probable error therefore again about 0.02.

The difference of the means of the two distributions is 0.183, and the probable error of the difference about 0.03. It is, therefore, significant. These last considerations have not included the results obtained with subject 6, because in this case a different process of calculation was used, viz. Urban's formula. The probable error for this formula has, however, been worked out¹ and can be calculated for the thresholds found for this subject. They show again that the results are significant when all three series are put together. Even each series by itself is almost sufficient. For example, the third series, given in the third column of Table 4, gives the following results²:

Period i	3.25 cms.	±	.218,
Period vi	2.29	„	± .228,
Difference	0.96	„	± .316.

In his extended experiments on lifted weights Urban found that there was no variation in the threshold (other than variation explicable as we say by chance) to be detected in a sitting of thirty-five judgments³. The criterion which he employed was to perform certain calculations (on a table resembling our Table 4, but divided into five periods only) which gave him the measures of the physical and accidental variation⁴. Similar calculations can be applied to our results, and it seems advisable to the writer to do so in an elementary way needing but little mathematical

¹ Urban's own calculation of the probable error needs correction. See G. H. Thomson, "The Probable Error of Urban's Formula," this *Journal*, 1913, vi. 217—222, especially equation (7). I have to thank Professor Urban for communicating to me privately his approval of these corrections and regret that his letter did not reach me in time to be mentioned in the article quoted.

² Note that these numbers confirm the probable errors given in column *B* of Table 5. For if 3.25 cms. be reduced to nearly unity, the p.e. .218 will be reduced to about .07; and as there are eight series the final p.e. should be of the order $.07 \div \sqrt{8}$. It is actually .022, which agrees very well.

³ F. M. Urban, *The Application of Statistical Methods to Psychophysics*, Philadelphia, 1908, p. 39.

⁴ Urban, *op. cit.* Table 25, p. 185, and pp. 34 ff. References are given to Lexis, Czuber, and Bortkewitsch.

manipulation, and calculated to make this criterion of variation clear to non-mathematical readers.

Column *A* in Table 5 gives the final results of all the experiments, and the point at issue is whether the numbers in this column actually vary with the periods, or whether the differences seen are due to chance alone and would disappear if larger averages could be taken. The probable error of each number is given, and of these the largest is .033, while the average is $.023 \pm .001$.

Now there are ten values in column *A*, with an average value of unity, and if these ten values are all measurements of the same quantity (that is, if there is no true variation with the time), then they ought to be so grouped about unity that the semi-interquartile range of this grouping is equal to the probable error of each. This semi-interquartile range, calculated by the formation and quadrature of the deviations, proves to be about $.043 \pm .007$, and is therefore considerably bigger than the probable error of any one of the numbers from which it is calculated, which lends support to the belief that these numbers do not differ by chance sampling only, but show also a fundamental difference not to be obliterated by averaging more and more readings. The difference between .043 and .023, however, is only barely significant in the light of the probable errors of these numbers, and these probable errors themselves are only vague in cases like these where the number of readings is only ten.

Lastly, if there is such a variation with time as the writer suggests, there ought to be some correlation between the different columns of Table 4, especially between those columns referring to the same subject. This is actually the case; for example, the correlation between the first two columns is $.465 \pm .167$. The correlation between some columns, and especially between the upper halves, is a good deal greater.

9. THE INFLUENCE OF THE EXPERIMENTER.

The writer believes that these experiments make it reasonably probable that, at least in the case of some subjects and experimenters, the spatial threshold during the course of a sitting at first sinks on the average and then later becomes erratic, but on the whole rises, except for an ultimate drop at the very end of the experiment due in all probability to 'end spurt,' for it was easy for the subject to judge that the sitting must be nearing its close, since all sittings were of the same length. It does not of course follow that these changes would occur

446 *Changes in the Spatial Threshold during a Sitting*

with every experimenter, and even if it proved to be so, it would still not be shown that the change is one which takes place in the subject: for it may be that the change is due to the conduct of the experimenter.

Almost all psychological tests consist in the response by a subject to stimuli arranged by an experimenter, and in many cases the result may well be a function of the mental and physical condition not only of the former, but also of the latter: and especially will this be the case where, as here, a considerable degree of dexterity is required. To test this point some experiments have been planned in which the writer's skill in using the aesthesiometer would be tested by mechanical means, but circumstances have not yet allowed these tests to be carried out. Against such an explanation of the results may be brought, firstly, the following general consideration. A change in the experimenter's dexterity during a sitting would probably consist of an increase of skill in the earlier part of the sitting. But such an increase of skill would probably make it more difficult for the subject to distinguish two points. For example, increase in the accuracy of applying the two points simultaneously would presumably make it more difficult for the subject to distinguish them. But this effect would be in exactly the opposite direction from that revealed by the present research.

There are a few further points which have bearing on this question. It has been already said that in some cases subject No. 6 acted as experimenter instead of the writer. The experiments on subject No. 8 (the writer himself) were thus carried out, and as they show little or no trace of the variation mentioned this might be taken as evidence that this variation takes place only when the writer himself acts as experimenter.

Another point which might be taken as further evidence in the same direction is the following. A number of the measurements on Nos. 6 and 7 were made on the same days and at the same hour, and the subjects were taken first on alternate days, now No. 6 first, next day No. 7 first. On these days the writer was always experimenter. There was considerable correlation (almost $.67 \pm .12$) between the daily mean thresholds of the two subjects, and this might be due to changes not in them but in the common experimenter.

On the other hand, the correlation might be due to similar weather conditions or some other common factor. Further, the absence of the sought-for variation in No. 8 might be due to an unconscious resistance owing to a strong desire to be impartial, for this subject alone knew exactly what the purpose of the experiments was. Finally, in

some experiments on subjects 7 and 9, the writer and subject 6 acted alternately as experimenter; and, as far as the necessarily smaller quantity of data enable a conclusion to be drawn, the suspected variation was as evident with the one experimenter as with the other—if anything perhaps it was more evident when No. 6 experimented. Consider, for example, the following numbers, which are proportional to the threshold for No. 9, and show results obtained by the writer and also by No. 6 as experimenter:

Subject No. 9. Averages of seven sittings. The experimenter was changed after each period.

Period	i	ii	iii	iv
Experimenter: writer ...	1·103	1·184	0·840	0·877
„ No. 6 ...	1·295	1·060	0·802	0·757

10. SUMMARY.

These experiments therefore appear to make the following results reasonably probable on the average.

- (1) During a sitting there is a change in the spatial threshold.
- (2) The threshold falls sharply at first, then slowly and steadily until about fifty judgments have been given. This improvement is probably due to the subject's 'finding himself.' Stimuli are compared with former stimuli, especially with those about which the subject felt certain. All the subjects made remarks during and after the sittings which suggested that this was so.
- (3) It then rises or at any rate becomes irregular.
- (4) There is a final drop just at the end of the sitting, due probably to 'end spurt.'

These changes are best shown in the figure (page 448), where the shaded area represents three times the probable error, on both sides of the central line.

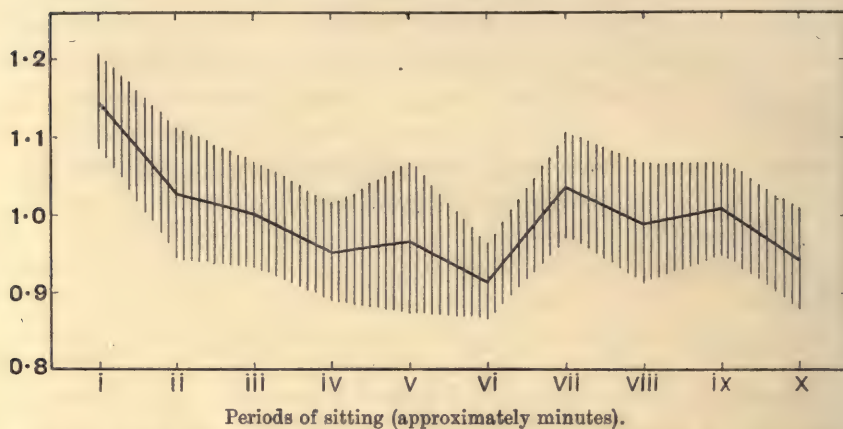
- (5) These changes seem to have their origin in the subject, not in the experimenter.

- (6) They appeared in greater or less degree in all the subjects except one, the writer himself.

448 *Changes in the Spatial Threshold during a Sitting*

Variation in the Spatial Threshold during one sitting.

Average of seventy-five sittings on four subjects. The Mean Threshold is taken as unity.



The shaded area shows three times the Probable Error, both above and below the average.

(Manuscript received 7 December 1913.)

REVIEW

Development and Purpose: an essay towards a philosophy of evolution.

By L. T. HOBHOUSE. London: Macmillan and Co. 1913. Pp. xxix + 383. 10s. nett.

Development and Purpose is the logical outcome and completion of the lines of thought set forth in Prof. Hobhouse's three previous works, *Mind in Evolution*, *Morals in Evolution*, and *The Theory of Knowledge*. In an interesting and important preface the author sketches in brief outline the development of his philosophical views. Rejecting Materialism and unable to accept Idealistic theories Prof. Hobhouse arrived at the conclusion "that a philosophy that was to possess more than a speculative interest must rest on a synthesis of experience as interpreted by science, and that to such a synthesis the general conception of evolution offered a key" (xviii). Still later it was seen "that the evolutionary process can be best understood as the effect of a purpose slowly working itself out under limiting conditions which it brings successively under control" (xxvi); and "that there is a spiritual element integral to the structure and movement of Reality" which, while not the whole of Reality, nevertheless becomes dominant by a process of evolution (xxvii).

Turning to the argument, Part I is, we find, chiefly devoted to an empirical survey. Its keynotes are 'correlation' and 'harmony.' All progress may be measured in terms of a more intimate, or a wider-embracing, correlation: while in the sphere of the Practical Reason, harmony is shown to be all-important, and a striving towards a more complete harmony the aim. In Part II the deductive argument is set forth and the validity of Experimental Reconstruction examined. This necessitates a review of the logic of induction in the course of which an important principle emerges, viz. that "the assumption of our scientific reasoning is that the variable relation is ultimately traceable to uniform relations, and that is to relations dependent on the intrinsic character of the terms 'as such'" (344).

As regards the author's conception of validity, we find that it too is saturated with the idea of development. For instance, "the validity of thought is not that of finality or achievement but of growth" (276), and again, a method may be valid even "though its immediate result does not possess final truth," it is an impulse in the right direction (339). The validity of such a method lies in the fact "that it is essential to the movement towards truth" (340); whence it follows that "validity is a wider conception than truth" (footnote to 128). The ultimate test of truth and validity alike seems to be that of coherence or consilience. Even axioms and first principles are not exempted it appears, since we are told that knowledge does not depend on first principles which are superior to criticism but on a body of mutually supporting judgments (265) which we build up as we go along (268). Nevertheless "What appear as 'first' principles are...not mere assumptions [but] they express the pervading unity in a system of judgments" (273); "Truth is objective" (274).

Empirical survey and deductive argument alike agree in demonstrating the presence of two processes in Reality. Mechanism exists alongside of Purpose; indeed, "the whole process of Reality is mechanically conditioned" (357). Hence Mechanism is not excluded from Reality (it is in fact one of the "modes in which Reality operates" footnote to 329), but "it must be conditioned by relation to an ultimate harmony, while harmony is equally conditioned by mechanism" (350). In the case of a psycho-physical organism "mechanical relations are qualified by teleological relations" thereby forming a new system (footnote to 329). The two processes, however, must not be falsely hypostatized as substances and a dualism of Body and Mind set up. The psycho-physical organism must rather be conceived as a unit system in which hypotheses of Interaction and of Psycho-physical Parallelism are alike equally out of place (*ibid.*). A criticism may here be raised and it may be asked whether the two processes, the mechanical and the teleological, are in very truth co-ordinate and mutually determining? For it would seem that since the mechanical order is ultimately imbued and transfused by the teleological order it is rather to be looked on as a condition of the latter, and in so far secondary and subordinate to it.

Fully understood the central essence of the Real is best expressed by *organicity* (338). Moreover, there is every reason to believe in the steady development of the organic principle. But "organic action is of mental character" (316). Therefore over and beyond the individual

minds familiar to the plain man Hobhouse postulates the permanent activity of a central Mind "not limited to a single physical organism" (365). This central Mind may be conceived as not fully defined at the beginning but as undergoing development (footnote to 370) commensurate with the increasing manifestation of Purpose (365). In the final stages its growth becomes largely self-consciously determined as the 'mother-sense' (comprising massed feeling and other underlying forces), together with the conditions determining the operation of its activity, are gradually brought within the focus of consciousness (38, 247): at the same time Mind is enabled to attain to a just orientation through the appreciation of its own development and limitations (280). It is, too, this very inclusion of the non-rational elements within the sphere of Reason which constitutes their redemption (249). Reality as a whole is rational; irrationalism is strongly denounced (145 *seq.*).

Again, the organic as such is harmonious; therefore the development of the organic principle implies a growth of harmony, which, eventually at least, is "identical with the growth of Mind" (364). It follows that until the whole of Reality is harmonised there is necessarily a certain degree of clash and discord, which we know as evil. This, unfortunately, is unavoidable since "Harmony alone does not explain existence" (350). The operation of mind is not absolute but conditioned. True, there is an increasing purpose in the universe, but purpose rightly understood is "a cause conditioned in its operation by its own tendency" (319). From this is deduced that though not every event is necessarily good *per se*, yet "every event proceeds from some combination of forces, each of which is somewhere or sometime necessary for the fulfilment of the world-purpose" (367); nevertheless, until this purpose is completely realised such elements "may remain in greater or less disharmony with other elements" (363-4). It might seem that overwhelming importance is here attached to the future. Further, since transitory evil, positive though it may be, is merely a condition (ultimately overcome) in the attaining of a completely established harmony, it seems open to question whether evil, according to Prof. Hobhouse's account, is so ultimate and real as he would postulate?

It will be clear that in a work of so comprehensive a scope many questions of the utmost interest come under consideration: of these space forbids one to treat in any detail. Suffice it to say that matter interesting to metaphysician, psychologist, biologist, aesthetician, theologian, moralist, logician and citizen alike is discussed.

"Time" we are told "depends on the function of change as necessary to development" (351). Beauty stands almost alone as "insusceptible of progress"; new art does not give greater beauty "but a fuller interpretation of experience with a deeper and more truthful expression of feeling" (218). In regard to the pre-formation theory, it is urged that "the germ need not be in the least like the matured order. It must only have a mode of operation, which is determined by the needs of that order" (360). Suggestions are thrown out for the origin of the animate from the inanimate (358) as well as for the rise of consciousness (287, 364). But it is in Part I that the matter chiefly interesting to the psychologist as such is to be found, where *inter alia* we are given a new criterion of consciousness, viz. sensori-motor activity (56 *seq.*).

It need scarcely be said in conclusion that the work is one of considerable interest and importance and demands individual study. It is especially valuable for its richness of suggestion and for the emphasis laid on empirical progress.

E. M. SMITH.

PUBLICATIONS RECENTLY RECEIVED

A Manual of Psychology. By Professor G. F. STOUT. Third Edition, revised and enlarged. London: University Tutorial Press, Ltd. 1913. Pp. xvii + 769. 8s. 6d.

In preparing this edition of his well-known work, Professor Stout has rewritten the greater part of it and has subjected the rest to careful revision. Two new chapters, on Instinct and Attention, have been added; and extensive alterations have been introduced in the account of the development of the Perception of External Objects, in the discussion of the connexion of Mind and Body, and in the treatment of the Perception of Spatial Relations. The effect of these changes is not only to widen very materially the scope of the book but also to render it far more easily intelligible to the general body of students.

Psychology applied to Legal Evidence and other Constructions of Law. By G. F. ARNOLD. Second Edition. Calcutta: Thacker, Spink & Co. 1913. Pp. 607. Rs. 12.

In this edition, various additions have been made and Münsterberg's book on *Psychology and Crime* has been freely drawn upon. But "as the author is unacquainted with German, he has been compelled to relinquish the idea of consulting" Stern's *Beiträge zur Psychologie der Aussage*, the *Zeitschrift für angewandte Psychologie*, and other foreign periodicals. Despite these defects and others arising from the training of the writer, the book is valuable as the first serious attempt of an English lawyer to consider the data and conditions of legal evidence from the standpoint of psychology.

Die agrammatischen Sprachstörungen: Studien zur psychologischen Grundlegung der Aphasielehre. Von Professor ARNOLD PICK. Berlin: Julius Springer. 1913. S. viii + 291. M. 14.

Nearly half of this book is devoted to a diffuse *Vorrede und Einleitung* in which the author lays stress, *inter alia*, on the value of psychological investigations, especially those of the Würzburg School, for the proper understanding of the pathology of speech defects. The subsequent six chapters deal with the definition of agrammatism, the definition of the sentence, the means of expression in speech, the path from thinking to speaking, Wundt's theory of *Gesammtvorstellung*, and internal speech. Thus the author treats his subject essentially from the standpoint of psychological theory and experiment. He dedicates the book "to the memory of Hughlings Jackson, the most profound thinker in neuropathology during the last century."

Objektive Psychologie oder Psychoreflexologie: die Lehre von den Assoziationsreflexen. Von Professor W. VON BECHTEREW. Translated from the Russian. Leipzig and Berlin: B. G. Teubner. 1913. S. viii + 468. M. 16; geb. M. 18.

This book is an attempt to found a psychology on reflexes; the following sentences will suffice to show its standpoint. Psychoreflexology, which treats of reactions in and for themselves, apart from the subjective experiences preceding or accompanying them, gives us, says the author "prose in place of poetry and treats the neuro-psychic functions exclusively from their external aspect." The author is convinced that as soon as exact experiment has determined the interrelation between the objective data of psychoreflexology and the data of subjective experience, psychology will become as exact a science as physics or chemistry. Only when psychoreflexology has reached the dignity of an exact science will subjective psychology rid itself of its present nebulous and metaphysical hypotheses.

Disturbances of the Visual Functions. By Professor W. LOHMANN. Translated by ANGUS MACNAB. London: John Bale, Sons & Danielsson, Ltd. 1913. Pp. 185. 15s. net.

This work contains a vast amount of useful material, but it can only be recommended to those who are in a position to realise its shortcomings. Many of the chapters are exceedingly sketchy; the paragraphs bearing on psychology are often inaccurate or insufficient; and the translation leaves much to be desired.

The Experimental Psychology of Beauty. By Dr C. W. VALENTINE. London: T. C. & E. C. Jack. (The People's Books Series.) N.D. Pp. 94. 6d.

The seven chapters of this little book are concerned with the beauty of colour, the beauty of form, balance and symmetry, and experiments with pictures. The author has included a number of his own experiments, many of which have not been previously published. A useful bibliography to each chapter is appended.

A Syllabus for the Clinical Examination of Children. By EDMUND B. HUEY. Baltimore: Warwick & York. Pp. 34.

A very useful series of blank forms is here given which have proved of value in recording and tabulating the conditions of the inmates of the Illinois State Institute for the Feeble-Minded. Four forms are recommended (1) for the home record (heredity, past history and environment), (2) for the teacher's or attendant's record (habits, capacities, morals, etc.), (3) for the physical examination, and (4) for the mental examination. The Binet-Simon tests are described and the special pictures originally used in connexion with these tests are reproduced.

Backward and Feeble-Minded Children. By Dr E. B. HUEY. Baltimore: Warwick & York. (Educational Psychology Monographs.) 1912. Pp. xii + 221. \$1.40.

The author gives a series of brief clinical studies of thirty-five 'border' cases of backward or feeble-minded children and feeble-minded adults. A still more intensive examination of mentally abnormal individuals on these useful lines is required to further our knowledge of mental deficiency.

Inductive versus Deductive Methods. By W. H. WINCH. Baltimore: Warwick & York. (Educational Psychology Monographs.) 1913. Pp. 146. \$1.25.

The writer describes a series of experiments planned to discover whether the inductive or deductive method gives the better result, (a) when the children are tested on precisely the same material as that which they had learnt or been taught, (b) when the children were examined by means of new material. The answer to the former of these problems varied in the five schools tested. But in all five cases "the children who were taught inductively did better work than those taught deductively," when they had to apply themselves to new material.

The Marking System in Theory and Practice. By I. E. FINKELSTEIN. Baltimore: Warwick & York. (Educational Psychology Monographs.) 1913. Pp. 88. \$1.

In this monograph the author sets out to determine the distribution of the marks awarded by different teachers, and the degree of unreliability which characterizes the percentage system at present in force in most American schools and colleges.

Les Origines de la Connaissance. Par Professor R. TURRO. Paris: Felix Alcan. 1914. Pp. 274. 5 fr.

The first two chapters of this book deal with the origin and nature of our experience of hunger. Trophic sensations and trophic perception, according to the author, are the forerunners of external perception; and the trophic effects of stimuli are ultimately responsible for our experiences of reality and causality.

Anglo-Indian Studies. By S. M. MITRA. London: Longmans, Green & Co. 1913. Pp. 525. 10s. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

- June 7, 1913¹. Memory and Consciousness, by A. ROBINSON.
Are Intensity Differences of Sensation Quantitative?, by C. S. MYERS, G. DAWES HICKS, H. J. WATT, and W. BROWN.
- June 8, 1913¹. Can there be Anything Obscure or Implicit in a Mental State?, by H. BARKER, G. F. STOUT, and R. P. HOERNLÉ.
- November 8, 1913. A Comparative Study of Normal and Sub-normal Children by means of Mental Tests, by A. R. ABELSON.
A Reaction Pendulum and a Disc, illustrative of Weber's Law, for use in Class Teaching (*demonstration*), by J. BROUGH.
Observations on the Process of Learning and Re-learning in Mice and Rats, by MARY E. MACGREGOR (introduced by Dr EDGELL).
An *a priori* Argument for the Existence of a Cerebral Centre for Affection, by A. WOHLGEMUTH.
- January 24, 1914. A Note on the Correlation of Ability and Variability, by WILLIAM BROWN.
General Ability and the Subjects of the School Curriculum, by Miss N. CAREY (introduced by Prof. SPEARMAN).
An Attempt at an Exact Study of Character, by EDWARD WEBB (introduced by Prof. SPEARMAN).

¹ In conjunction with the Aristotelian Society and the *Mind* Association.

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